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A

JOURNAL

OF

NATURAL PHILOSOPHY, *CHEMISTRY,* AND THE ARTS.

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NEW-YORK.

VOL. IX.

Illustrated with Engravings.

BY WILLIAM NICHOLSON.

LONDON:
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THE AUTHOR,
AND SOLD BY HIM AT NO. 10, SOHO-SQUARE;
AND BY
J. MURRAY, NO. 32, FLEET-STREET.

1804.

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the herbage and ſmall thiſtles, no higher than the graſs, did not appear to have been ſinged at all. Thoſe in the centre, for about three yards, ſeemed much leſs injured than thoſe nearer the circumference. In two places the ſoil was turned up, a few feet in length and an inch or two in depth; and where the ground ceaſed to be broken up, one might perceive that the lightning had glanced, with ſeveral ramifications, along the ſurface, but under the long graſs, leaving a track, ſuch as moles and mice ſometimes make. In one place, where the impreſſion made upon the ſoil was deepeſt, and ſomewhat reſembled the letter V, a ſtraight round hole appeared at the angle, which was about two feet deep, and about three inches in diameter.

Probability of a ſtone having fallen from the atmoſphere.

After I had left the place, it occurred to me that this hole might have been produced by a ſtone falling from the clouds, a phenomenon which has lately cauſed much ſpeculation. The more I thought of this, the more probable it ſeemed. The hole was round and perpendicular, at no great diſtance from the end of the barn, and a tall tree was alſo near the place; either of which was more likely to attract the lightning than the plain field. A very lofty tower, which was ſtruck a few years ago, was about three times the diſtance of the tree. The thunder was not ſo loud as when the tower was ſtruck, but the ſmell of ſulphur was much ſtronger. Theſe conſiderations determined me to return the firſt opportunity and examine the place more thoroughly.

On examination none was found.

From one cauſe or other I could not do this conveniently before yeſterday evening, when the farmer and his ſon very obligingly went with me, and dug a circular pit about four feet in diameter, having the hole in its centre. At the depth of two feet they came at a ſhivery rock of grit, in which I hoped to find the expected ſtone imbedded; but when the ſoil was cleared away level with the bottom of the hole, there was no ſuch thing; nothing appeared but ſmall, oblong, flat pieces of gritſtone, through the moiſt crevices of which the electric fluid probably eſcaped. Though diſappointed in the principal part that I hoped to communicate, I reſolved to ſend you the above, as I think the lightning very ſeldom deſcends ſo far into ſolid ground, unleſs when guided by ſome particular conductor. If the account afford you any information or amuſement, I ſhall be glad; and ſhould the ſubject merit any of your queries, they will be received with pleaſure by

Your humble ſervant,
JONATHAN WILSON.

Remarks on the above.

Whether fairy-rings be thus formed.

THE firſt part of Mr. Wilſon's letter countenances Dr. Darwin's explanation of fairy-rings; for of all the thiſtles included in the electrified circle, thoſe in the centre received the leaſt injury. The hypotheſis of Dr. Darwin has always ſeemed to me to labour under unſurmountable difficulties; and this is the reaſon why I point out a fact which eſtabliſhes an exact agreement of the explanation, and natural appearances, in one inſtance.

Compariſon with Dr. Hutton's obſervations of the fairy-rings on Arthur's Seat.

Fairy-rings are permanent objects, the nature of which is but imperfectly underſtood; and it is to be wiſhed that Mr. Wilſon would find leiſure to obſerve and deſcribe the future conſequences of the electric diſcharge at Biddulph Hall. The herbage of the ramified figure mentioned in the letter, will in all likelihood die in the courſe of a few weeks; becauſe plants that have ſuſtained ſtrong electrical ſhocks, ſeldom ſurvive the operation more than a month or two. Should this change take place in the circle, it will give a new appearance to the ſurface of the ground, by expoſing to view the branched path of the lightning; nor is it improbable but that this alteration will be ſucceeded by another of a more ſingular kind. There is reaſon to ſuſpect, that each ramification of the track will be again covered with a lively verdure the next ſpring, being accompanied at the ſame time by a contiguous patch of blaſted herbage reſembling its own figure. This ſuſpicion is countenanced by certain obſervations of Dr. Hutton of Edinburgh, who remarked, that the fairy-rings upon Arthur's Seat annually increaſe in diameter; *i. e.* the withered circumference of each circle becomes green in ſpring, and is ſurrounded in a ſhort time by a freſh ring of a ruſſet colour. We have no right to diſpute the juſtice of Dr. Hutton's obſervations, but the queſtion is undetermined, whether the appearance is conſtant, or reſults from the nature of the ſoil. The peruſal of the foregoing remarks will perhaps admoniſh my friend Mr. Wilſon, that Fortune has furniſhed him with an opportunity of proſecuting the inquiry, ſuch as ſhe rarely affords to perſons of correct obſervation. I will not treſpaſs upon your pages by a comment on the remaining facts of my friend's letter; let them ſpeak for themſelves; in particular, the preference of a thunder cloud to low ground, in the preſence of lofty objects, including a tall tree.

JOHN GOUGH.

ANNOTATION.

I BELIEVE the opinion that fairy-rings are cauſed by lightning, is of conſiderable antiquity. Dr. Prieſtley gave ſupport to this concluſion by his experiment of the concentric rings formed on a poliſhed metallic ſurface by the exploſion of a battery. An effect of this kind produced in Kenſington Gardens, is deſcribed, with an engraving, in our Quarto Series, Vol. I. p. 546.

W. N.

II.

On the Computation of Tables of Squares and Cubes. In a Letter from E. O.

To Mr. NICHOLSON.

SIR,

Computation of ſquares and cubes.

I SHOULD not have troubled you with any remarks on the method of computing ſquares and cubes, if I had not ſeen the paper which was printed on that ſubject in your Journal for laſt July. But as it appears, from that letter, that there may be perſons who would like to employ themſelves on theſe calculations, it is deſirable that the plaineſt and eaſieſt methods ſhould be pointed out to them.

Every one who is accuſtomed to calculation, is acquainted with the advantage of conſtantly repeating the ſame operations. When the reſult of any calculation is to be made out, in ſome parts by multiplication and in others by diviſion, ſubtraction, or addition, it is impoſſible for the moſt unwearied diligence to avoid occaſional miſtakes. It is right, therefore, to ſimplify as much as poſſible in this reſpect; and if the calculation

Computation of ſquares and cubes.

lation cannot be effected by a ſingle operation, I believe I ſhall be joined by all perſons accuſtomed to arithmetical computation, in recommending the uſe of addition[*] in preference to ſubtraction, and of multiplication in preference to diviſion. Your correſpondent H. G. does not ſeem to have paid ſufficient attention to theſe conſiderations; and I fear that he not only would be ſoon wearied, if he were to work by the method which he recommends, but (what is of ſtill more conſequence) he could not depend for any continuance upon the accuracy of his computations.

The only method by which an extenſive table like Mr. Councer's could have been calculated, muſt have been by the conſtant addition of differences; and the rules for this method may be eaſily deduced from the following conſiderations: Let $x+3a$, $x+2a$, $x+a$, &c. be any numbers in arithmetical progreſſion. Then by the binomial theorem,

	1ſt *Differences.*	2d *Differences.*
$\overline{x+3a}\vert^{2}=x^{2}+6ax+9a^{2}$		
	$2ax+5a^{2}$	
$\overline{x+2a}\vert^{2}=x^{2}+4ax+4a^{2}$		$2a^{2}$
	$2ax+3a^{2}$	
$\overline{x+a}\vert^{2}=x^{2}+2ax+a^{2}$		$2a^{2}$
	$2ax+a^{2}$	
$x^{2}=x^{2}$		

It will be foreign from my preſent purpoſe to enter into a particular conſideration of the manner in which theſe differences ariſe. It will be ſufficient to remark, that the ſecond difference is conſtant, and that its value will be always 2, when we conſider a as $=1$, or the progreſſion $x+3a$, $x+2a$, &c. as a ſeries of common numbers differing from one another by unity. Hence the firſt differences of the ſquares of ſuch numbers become themſelves an arithmetical progreſſion, of which the common difference is 2; and conſequently, if we know the firſt difference between the ſquares of any two ſuch numbers, we can find the difference between the ſquares of the two next.

---

[*] If this opinion wanted any argument in its favour, I might ſupport it by the authority of that able mathematician Captain Mendoza, who has calculated his new tables in ſuch a manner as to make all the equations additional.

What

Continuation of ſquares and cubes.

What has been ſaid may be exemplified by applying it to the continuation of Mr. Councer's ſquares. For

$$\begin{array}{l} 798684121 \text{ is the ſquare of } 28261, \\ 798627600 \text{ is the ſquare of } 28260, \\ \hline \end{array}$$

56521 is their difference, therefore 56523 is the difference between the ſquare of 28261 and the ſquare of 28262. By the addition in every inſtance of 2, we may find the ſeries of differences; and by the conſtant addition of them, we may eaſily find the ſquares. Thus

$$\begin{array}{r} 798684121 = \overline{28261}|^2 \\ 56523 \phantom{= \overline{28261}|^2} \\ \hline 798740644 = \overline{28262}|^2 \\ 56525 \phantom{= \overline{28261}|^2} \\ \hline 798797169 = \overline{28263}|^2 \\ 56527 \phantom{= \overline{28261}|^2} \\ \hline 798853696 = \overline{28264}|^2 \\ 56529 \phantom{= \overline{28261}|^2} \\ \hline 798910225 = \overline{28265}|^2 \end{array}$$

By a ſimilar method the table of cubes might be continued by the addition of differences; but, in this caſe, the calculation will be a little more complicated: Becauſe, if the terms of any arithmetical progreſſion be raiſed to the nth power, there will be n orders of differences, and the laſt will be the only one of them which will be conſtant. For the cube, therefore, where $n = 3$, we ſhall have one order of differences more than for the ſquare.

As before, let the progreſſion be $x + 3a$, $x + 2a$, $x + a$, x, &c.

Computation of squares and cubes.

	1ſt Difference.	2d Difference.	3d Diff.
$\overline{x+4a}^3 = x^3 + 12ax^2 + 48a^2x + 64a^3$			
	$3ax^2 + 21a^2x + 37a^3$		
$\overline{x+3a}^3 = x^3 + 9ax^2 + 27a^2x + 27a^3$		$6a^2x + 18a^3$	
	$3ax^2 + 15a^2x + 19a^3$		$6a^3$
$\overline{x+2a}^3 = x^3 + 6ax^2 + 12a^2x + 8a^3$		$6a^2x + 12a^3$	
	$3ax^2 + 9a^2x + 7a^3$		$6a^3$
$\overline{x+a}^3 = x^3 + 3ax^2 + 3a^2x + a^3$		$6a^2x + 6a^3$	
	$3ax^2 + 3a^2x + a^3$		
$x^3 = x^3$			

If therefore we make $a = 1$, the third difference will be conſtantly 6, and the ſecond difference will become $6x + 18$, $6x + 12$, $6x + 6$; ſo that if any three numbers be taken in an arithmetical ſeries (in which $a = 1$), the ſecond difference of their cubes will be conſtantly ſix times the middle number.

For the ſake of example, we may apply theſe obſervations to the continuation of Mr. Councer's cubes. We may conſider 26559, 26560, 26561, as part of an arithmetical ſeries: by the table we could find the difference of the cubes of 26559 and

Computation of ſquares and cubes.

and 26560, and, by what has been obſerved above, the ſecond difference of the three cubes will be* $6.26560 = 159360$; which, added to the difference between the cubes of 26559 and 26560, will give the difference between the cubes of 26560 and 26561. So far we ſhould proceed exactly in the ſteps which are traced out by your correſpondent H. G.; but I differ from him in this reſpect, that I ſhould by no means recommend this method for the ſolitary calculation of any particular cube: it is only mentioned here for the ſake of ſhewing how to aſcertain the numbers by which we muſt commence the ſeries of differences, when we want to continue a table which has already been calculated to a certain extent. For the third difference is conſtantly 6; therefore if we take an arithmetical progreſſion, of which the terms differ from one another by that number, and which begins with 159360 (ſuch as 159360, 159366, 159372, *&c. &c.*), we ſhall aſcertain the ſecond differences of the cubes which we want to find. Theſe ſecond differences muſt be added to the reſpective firſt differences, and, by that means, we ſhall find a ſeries of numbers, which, added to each particular cube, will give us the cubes of the numbers next above it. Thus

$\overline{26560}|^3 = 18736316416000$

$\overline{26559}|^3 = 18734200194879$

$2116221121 = \overline{26560}|^3 - \overline{26559}|^3$

159360 2d Difference.

$2116380481 = \overline{26561}|^3 - \overline{26560}|^3$

159366 2d Difference.

$2116539847 = \overline{26562}|^3 - \overline{26561}|^3$

159372 2d D.

$2116699219 = \overline{26563}|^3 - \overline{26562}|^3$

159378 2d D.

$2116858597 = \overline{26564}|^3 - \overline{26563}|^3$

159384 2d D.

$2117017981 = \overline{26565}|^3 - \overline{26564}|^3$

Having thus aſcertained the firſt differences, we may proceed to add them to the cubes:

---

* The dot (.) between the figures is here uſed as the ſign of multiplication. N.

Computation of ſquares and cubes.

18736316416000 = $\overline{26560}|^3$
2116380481 1ſt Diff.

18738432796481 = $\overline{26561}|^3$
2116539847 1ſt D.

18740549336328 = $\overline{26562}|^3$
2116699219 1ſt D.

18742666035547 = $\overline{26563}|^3$
2116858597 1ſt D.

18744782894144 = $\overline{26564}|^3$
2117017981 1ſt D.

18746899912125 = $\overline{26565}|^3$

The methods here ſtated are, probably, the eaſieſt which can be deviſed for conſtructing an extenſive table: but it muſt frequently happen, that the calculator will want the ſquare or cube of ſome number which is greater than any which is contained in the table. It may be uſeful, therefore, to conſider the aſſiſtance which the table may afford him in facilitating the computation.

It is well known, that $x^m \times y^m = \overline{xy}|^m$.* Therefore, if we want to find the ſquare of a number which is e double of any contained in the table, we have only to multiply the given ſquare by 4. In the ſame manner, if we want to find the ſquare of a number which is exactly three or four times as great as any contained in the table, we may find it by multiplying the given ſquare by 9 or 16. Thus, for example,

16522531600 = the ſquare of 128540;
4

660901264400 = the ſquare of 257080;

16522274521 = the ſquare of 128539;
9

148700470689 = the ſquare of 385617.

---

[*] I do not know whether it is worth while to mention the circumſtance, but your correſpondent H. G. has made a miſtake in the application of this rule. For he ſays that, if we "multiply the cube of any given root by 8, the product will be the cube of twice the next root:" whereas the product will be the cube of twice the given root.

In

Computation of ſquares and cubes.

In this manner we may find the ſquares of all even numbers which do not exceed the double of the table: we may alſo find the ſquares of numbers, to a greater extent, which are multiples of 3: the multiples of 4 and 5 may be ſquared likewiſe, if we multiply by 16 and 25; but ſquare numbers increaſe very rapidly, and conſequently this method can be only uſed (with advantage) for the ſmall multiples of the tabular numbers. We muſt, therefore, conſider ſome other method which may be applicable to prime numbers and multiples of large ones.

The ſquare of $2x + 1 = 4x^2 + 4x + 1 = 2x^2 + 4x + 2 + 2x^2 - 1 = 2.\overline{x+1}|^2 + 2x^2 - 1$, or $2.\overline{\overline{x+1}|^3 + x^2} - 1$. If, therefore, we want to find the ſquare of an odd number which does not exceed the double of thoſe which are ſquared in the table, we muſt divide the next even number below it by 2, and this half will be equivalent to x in the above equation; the next number above this half will give us $x + 1$. Having theſe two ſmaller numbers, we may find their ſquares by the table; and the ſum of their ſquares multiplied by 2, will exceed the ſquare required by 1. Thus, if it were required to find the ſquare of $257079 = 2.128539 + 1$: Here $x = 128539$, and $x + 1 = 128540$; therefore, by Mr. Councer's table, the ſquare of 128540 would be 16522531600
the ſquare of 128539 would be 16522274521

$$\begin{array}{r} 16522531600 \\ 16522274521 \\ \hline 33044806121 \\ 2 \\ \hline 66089612242 \end{array}$$

Therefore the ſquare of 257079 is 66089612241.

Inſtead of examining each particular caſe, when the number to be ſquared is more than the double of thoſe in the table, it will be beſt to conſider the theorem, from which a general rule may be deduced. $\overline{nx+a}|^2 = n^2x^2 + 2anx + a^2 = nx^2 + 2anx + na^2 + \overline{n^2-n}.x^2 + \overline{1-n}.a^2 = n.\overline{x+a}|^2 + n.\overline{n-1}.x^2 + \overline{1-n}.a^2 = n.\overline{\overline{x+a}|^2 + \overline{n-1}.x^2} + \overline{1-n}.a^2$. Hence we muſt multiply x^2 by $\overline{n-1}$, to the product add $\overline{x+a}|^2$, multiply the ſum of theſe two quantities by n, and this laſt product will exceed the ſquare required by $\overline{n-1}.a^2$. The only caution neceſſary, is in dividing the given number by x; for as we ſuppoſe it equal to $nx+a$, we ſhould make x as great as it poſſibly can be, without being ſo large, that when added to the remainder a (as $x+a$), it ſhould exceed the numbers of which the ſquares are given in the table. Becauſe n and a are the only numbers by which we have to multiply, and as a muſt be leſs than n, we ſhall always have them as ſmall as poſſible, if we take x according to the directions here given. If, for example, it be required to find the ſquare of 385618, which is greater than twice, and leſs than three times 128540 (the greateſt number which is ſquared by Mr. Councer's table); here $385618 = 3.128539 + 1$; therefore if $n = 3$, $x = 128539$, and $a = 1$, $x+a$ will be $= 128540$, and will be within the extent of the table: Hence

Computation of ſquares and cubes.

x^2, or the ſquare of 128539, would be by the table 16522274521
$n-1$, or $3-1$ - - 2

33044549042
$\overline{x+a}|^2$, or the ſquare of 128540, would be 16522531600

49567080642
n or 3 - - 3

148701241926
and as $\overline{n-1}.a^2 = 2$, the ſquare of 385618 will be 148701241924

If the ſquare required were that of 385619, in this caſe $a=2$, and conſequently, if $x = 128539$, $x+a$, or 128541, would exceed the extent of the table: therefore n muſt, in this caſe, be equal to 4, $x = 96404$, and $a = 3$, and

the ſquare of 96404 = 9293731216
3

27881193648
the ſquare of 96407 = 9294309649

37175503297
4

148702013188; but $\overline{n-1}.a^2 =$ 3.9, or 27; therefore the ſquare of 385619 is 148702013161.

From this example we ſee the neceſſity of being careful that $x+a$ does not exceed the numbers in the table: at the ſame

Computation of ſquares and cubes.

ſame time it may be remarked, that this is an extreme caſe, which can never occur but under particular circumſtances: for it only happens when the number to be ſquared is leſs than a multiple of the higheſt number in the table, and greater than the ſame multiple of the number next leſs than the higheſt; that is, for a table like Mr. Councer's, it muſt be leſs than $m.128540$, and greater than $m.128539$: under theſe circumſtances, if a be greater than 1, the caſe will occur which is the ſubject of the above caution.

It remains for us to conſider the method of finding the cubes of numbers which exceed thoſe given in the table.

Upon the principle to which we referred before, that $x^m \times y^m = \overline{xy}|^m$, we may find the cube of a number which is a multiple of any one contained in the table, by ſimple multiplication: for the cube of any number multiplied by 8, will give the cube of double that number; the cube multiplied by 29, 64, or 125, will give us the cube of 3, 4, or 5 times the number: Thus,

$$\begin{array}{r l}
3368928644271 & = \text{the cube of } 14991 \\
8 & \\
\hline
26951429154168 & = \text{the cube of } 29982. \\
 & \\
1045678375000 & = \text{the cube of } 10150 \\
27 & \\
\hline
7319748625000 & \\
20913567500000 & \\
\hline
28233316125000 & = \text{the cube of } 30450.
\end{array}$$

But as the cubes increaſe more rapidly even than the ſquares, it will ſtill be more neceſſary in this caſe than in the former, to eſtabliſh ſome means of finding the cubes of thoſe high numbers which are either prime or not exactly a ſmall multiple of a number contained in the table; and by proceeding in a manner ſimilar to that which we uſed for the ſquares, we eaſily eſtabliſh a general rule for this purpoſe: For $\overline{nx+a}|^3 = n^3x^3 + 3an^2x^2 + 3a^2nx + a^3 = nx^3 + 3nax^2 + 3na^2x + na^3 + \overline{n^3-n}.x^3 + \overline{3an^2-3an}.x^2 + a^3 - na^3 = n.\overline{x+a}|^3 + n.\overline{n^2-1}.x^3 + n.\overline{n-1}.3ax^2 + \overline{1-n}.a^3 = n \times \overline{x+a}|^3 + \overline{\overline{n-1}.\overline{n+1}\,x^3 + 3ax^2}| + \overline{1-n}.a^3$. Determine, therefore, n, x, and a, in the ſame

manner

Computation of ſquares and cubes.

manner as for the ſquare, and be equally careful that $x+a$ does not exceed the numbers in the table; add $\overline{n+1}\,.\,x^3$ to $3ax^2$; multiply the ſum of theſe two quantities by $n-1$; to the product add $\overline{x+a}\big|^3$, and multiply this ſum by n: this laſt product will exceed the cube required by $\overline{n-1}\,.\,a^3$.

If, for example, it be required to find the cube of 53119, or $2\,.\,26559+1$. Then $n=2$, $a=1$, $x=26559$, and $x+a=26560$, and the cube of $53119=2\times\overline{\overline{26560}\big|^3+3\,.\,\overline{26559}\big|^3+3\,.\,\overline{26559}\big|^2}-1$: Therefore, by Mr. Councer's table, the cube of 26559 would be 18734200194879

$$\begin{array}{lr}
\text{The ſquare of } 26559 \quad - & 705380481 \\
\hline
 & 18734905575360 \\
 & 3 \\
\hline
 & 56204716726080 \\
\text{The cube of } 26560 \quad - & 18736316416000 \\
\hline
 & 74941033142080 \\
 & 2 \\
\hline
 & 149882066284160 \\
\text{Therefore the cube of } 53119 \text{ is} \quad - & 149882066284159
\end{array}$$

Laſtly, let it be required to find the cube of 79601, or $3\,.\,26533+2$: here $n=3$, $a=2$, $x=26533$, $x+a=26535$; therefore, by what has been demonſtrated, $\overline{79601}\big|^3=3\times\overline{\overline{26535}\big|^3+2\times4\,.\,\overline{26533}\big|^3+6\,.\,\overline{26533}\big|^2}-2\,.\,8$.

$$\begin{array}{rr}
\overline{26533}\big|^2=704000089 & \overline{26533}\big|^3=18679234361437 \\
3 & 2 \\
\hline
2112000267 & 37358468722874 \\
 & 2112000267 \\
\hline
 & 37360580723141 \\
 & 4 \\
\hline
 & 149442322892564 \\
 & \overline{26535}\big|^3=18683458680375 \\
\hline
 & 168125781572939 \\
 & 3 \\
\hline
 & 504377344718817 \\
\text{Therefore the cube of } 79601 \text{ is} \quad - & 504377344718801
\end{array}$$

E. O.

Analyſis

III.

Analyſis of a Triple Sulphuret of Lead, Antimony, and Copper, from Cornwall. By CHARLES HATCHET, *Eſq. F. R. S. From the Philoſophical Tranſactions for* 1804.

Hiſtory of the mineral.

THE ſubſtance which forms the ſubject of this paper, has hitherto been regarded as an ore of antimony; it is extremely rare, and has only been obtained from Huel Boys, in the pariſh of Endellion, a mine which, from deficiency of profit, has for ſome time been abandoned.

The ſcarcity of the ore has probably been the cauſe of its being unknown to foreign mineralogiſts; indeed few even of the Britiſh cabinets poſſeſs it; but the moſt perfect and beautiful ſpecimens are (as far as I know) to be ſeen in the ſplendid collection of Philip Raſhleigh, Eſq. of Menabilly, in Cornwall.

To Mr. Raſhleigh we are indebted for the firſt deſcription of this ore[*]; but no ſubſequent notice had been taken of it, until the preceding paper was written by the Count de Bournon, whoſe eminent merits, as a mineralogiſt and cryſtallographer, are well known to this Society.

I.

Sp. gravity.

The ſpecific gravity of this ſubſtance is 5766, 65° of Fahrenheit.

II.

Heat by the blow-pipe expelled ſulphur and white fumes, and left ſulphuret of lead and metallic copper.

If ſuddenly heated on charcoal, by the blowpipe, it crackles and ſplits; but, when gradually expoſed to the flame, it liquifies, and, upon cooling, aſſumes a dull metallic grey colour.

When the globule was longer expoſed to heat, white fumes (which at firſt had a ſulphureous odour) were evolved, and partly ſettled on the charcoal.

Ebullition prevailed during the diſcharge of theſe white fumes; and the globule gradually ſuffered conſiderable diminution, remaining at length tranquil, and of a very dark gray colour.

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[*] Specimens of Britiſh Minerals, ſelected from the Cabinet of Philip Raſhleigh, Eſq. F. R. S. &c. Part I. page 34, Plate XIX.

Upon examination, this appeared to be principally ſulphuret of lead, which, like a cruſt, enveloped a minute globule of metallic copper, ſo malleable as to bear to be flattened by a hammer.

III.

Some of the ore, finely powdered, was put into a matraſs, and nitric acid diluted with an equal portion of water was poured on it. Upon being digeſted in a low heat, a conſiderable part, was diſſolved, with much efferveſcence. Some ſulphur, which floated, was ſeparated; and the clear liquor, which was bluiſh green, was decanted from the reſiduum at the bottom of the veſſel.

Digeſtion with dilute nitrous acid, &c. indicated ſulphur, lead, copper, and antimony, with a little iron.

A great part of the exceſs of acid being expelled from the ſolution, it was largely diluted with diſtilled water, and ſome diſſolved muriate of ſoda was added; but this did not produce any alteration in the tranſparency of the liquor. A ſolution of ſulphate of ſoda was then poured in, and formed a very copious precipitate of ſulphate of lead.

When this had been ſeparated, the liquor was ſaturated with ammonia; by which it was changed to a deep blue colour. A few flocculi of iron were ſeparated; and the remainder was found to contain nothing but copper.

The ſulphur which had floated, was added to the reſiduum which had ſubſided to the bottom of the matraſs; and the whole was digeſted with muriatic acid. This ſolution was of a ſtraw colour; and, when ſeparated from the ſulphur, and poured into a large quantity of water, afforded a plentiful white precipitate.

This precipitate was completely reſolved into white fumes, by the blowpipe; and the muriatic ſolution of it, when added to water impregnated with hydro-ſulphuret of ammonia, formed the orange coloured precipitate, commonly known by the appellation of golden ſulphur of antimony.

IV.

Muriatic acid did not immediately act upon the pulverized ore; but a ſolution was ſpeedily effected by the addition of a few drops of nitric acid: pure ſulphur was ſeparated; and the liquor, being decanted into water, yielded a copious precipitate of oxide of antimony.

The ſame reſults by treatment with muriatic acid and a little nitric.

from muriatic acid by water, is to metallic antimony as 130 to 100; therefore, the 63 grains of the oxide muſt be eſtimated at 48.46, grains of the metal.

Again, ſulphate of lead is to metallic lead as 141 to 100; therefore, 120.20 grains of the former are = 85.24 grains of the latter. And, laſtly, black oxide of copper contains 20 *per cent.* of oxygen; conſequently, 32 grains of the black oxide are = 25.60 grains of metallic copper.

Component parts.

The proportions for 200 grains of the ore, will therefore be,

Sulphur		34.
Antimony		48.46
Lead		85.24
Iron		2.40
Copper		25.60
		195.70
	Loſs	4.30

Or, *per cent.*

Sulphur		17.
Antimony		24.23
Lead		42.62
Iron		1.20
Copper		12.80
		97.85
	Loſs	2.15

Theſe proportions, I have reaſon to believe, are tolerably exact; for I did not obſerve any eſſential variation in the reſults of two other analyſes, which I made of this ſubſtance, with every poſſible precaution.

The loſs may be principally aſcribed to the oxide of antimony and ſulphate of lead; but eſpecially to the former, which has a great tendency to adhere to filters and glaſs veſſels.

In ſome of the preliminary experiments, I obtained a ſmall portion of zinc; but, having received, through the kindneſs of Mr. R. Phillips, of Lombard-ſtreet, ſome pure cryſtals of the ore, I found that the zinc had proceeded from blende, which was imperceptibly mixed in the ſpecimens which I had firſt examined.

IV.

Deſcription of a compound Electrical Inſtrument for condenſing and doubling: with Experiments. By Mr. Wilson.

London, Auguſt 10, 1804.

To Mr. NICHOLSON,

SIR,

Compound electrical condenſer.

I TAKE the liberty of troubling you with the following account and drawing of a compound condenſer of electricity, which I think is an improvement on Cavallo's multiplier of electricity, deſcribed in his Treatiſe on Electricity in the third volume, of the fourth edition, and if you think it worth a place in your Philoſophical Journal, you will very much oblige me by inſerting it therein.

I am your obedient
Humble ſervant,
W. WILSON.

Deſcription and drawing of the inſtrument.

The drawing is an exact repreſentation of the inſtrument, about half its real ſize, *(Plate 3.)* The plates A, B, C, D, and E are ſupported by glaſs ſticks, covered with ſealing wax at the upper part; but the plate F is ſupported by a wire, which has a joint at the bottom, by which it may be brought near the plate E, or thrown back away from it. 3 is a ſcrew, which ſtops againſt the glaſs ſupport of E, and regulates the diſtance of the two plates when one is brought near the other. The plates A, C and E are fixed to the bottom board, and B and D to the levers L l, which move round the pins p p, and and are connected by the rod R. It will be ſeen by the drawing, that when the lever L is moved, the plates B and D are moved in contrary directions, that is, they both approach towards or recede from the plate C at the ſame time, but on oppoſite ſides. The faces of the plates are ground flat, and they are ſo adjuſted that if the ſcrews 2 2 did not ſtop the levers when they are moved, the face of B would ſtop flat againſt the face of A, and ſo would the face of D againſt C. The ſcrews 2 2 ſtop them when at a very ſmall diſtance, which may be made more or leſs by turning the ſcrews.

Its uſes as a condenſer, a ſingle and double multiplier, and a doubler of electricity.

This inſtrument ſerves the purpoſe of a condenſer, a ſingle and double multiplier, and a doubler of electricity. When it is uſed as a ſingle condenſer, the plate B is brought as near A as the ſcrew 2 will permit, and then the wires *b* and *f* touch one another and uninſulate B, which increaſes the capacity of A. As the capacity of A is increaſed it will receive a greater charge of electricity from any electrified body brought into contact with it. Conſequently, when B is removed, the charge on A will be much more intenſe than it would have been if the plate B had not been oppoſed to it; but this intenſity will not be greater than that of the electrified body, if its ſurface is not greater than the ſurface of the receiving plate A; for a condenſer does not increaſe the quantity of electricity, it only collects it into a ſmaller ſpace than it was before. So that if the electricity of a very ſmall body is required to be aſcertained, the ſimple condenſer will not anſwer the purpoſe. Some other means muſt be uſed when this is the caſe, and Cavallo's multiplier is the leaſt exceptionable of the inſtruments uſed for this purpoſe. The inſtrument repreſented in the drawing forms a multiplier, either ſingle or double, for when B is near A and uninſulated, A will receive a much greater charge that it would if B had not been near. And becauſe B is uninſulated, A will induce a contrary ſtate on B, and of nearly equal intenſity; which ſtate is preſerved: For the inſtant the lever is moved to carry B farther from A; the contact between the wires *b* and *f* is diſcontinued, and B inſulated, and as it removes farther away from A, the intenſity of its charge will increaſe the ſame as that of A will, and be ready to part with nearly the whole to another condenſer.

Now as the plate B removes away from A, the plate D approaches C, and when at a ſmall diſtance from it, the wires *d* and *g* come into contact, which operation uninſulates D, and therefore cauſes the two plates C and D to form a condenſer; and at the ſame inſtant that *d* touches *g*, the wires *b* and *c* touch one another, ſo that nearly the whole of the charge of B is communicated to C, which induces a contrary ſtate on D, as the charge of A did on B. D will therefore be in the ſame ſtate as A is, which ſtate is preſerved; for the inſtant the plate B removes away from near C, the plate D alſo removes away from it, and the contact between the wires *d* and *g* is diſcontinued, and D inſulated; and by continuing the motion the plate

Its uſes as a condenſer, a ſingle and double multiplier, and a doubler of electricity.

plate B will come near A, and D will remove away from C till the wires *b* and *f* touch one another, at which inſtant the wire *d* touches the wire *e*. Now if the plate E and F are near one another, they will form a third condenſer, and nearly the whole of the charge of D is communicated to E in the ſame manner the charge of B was communicated to C; at the ſame time B recruits its charge by coming near A, which will be communicated to C, which induces a contrary ſtate on D, and which D communicates to E, and this operation is repeated every time the lever is moved backwards and forwards; ſo that the charge on C is increaſing at every motion of the lever, while the charge on A remains the ſame; and when C becomes ſo much charged as not to be capable of receiving any more from B (which will be the caſe in a certain number of motions) the operation will ſtill go on between D and E, the charge on E increaſing while the charge on C remains the ſame, ſo that E will acquire a charge as much greater than C as the charge on C is greater than the charge on A, which will be manifeſted by removing the plate F away from E. When the wire *a* is ſcrewed into the plate A, and connected with E by means of the wire 4 5; the inſtrument then poſſeſſes all the properties of the double of electricity, for then all the charge communicated to E (which is of the ſame nature as that of A) will be communicated to A, which will continually increaſe the intenſity of its charge, and that will have an increaſed effect on B, &c. at every motion of the lever. There is no limits to this accumulation, but that where the charge is ſo intenſe as to paſs from one plate to the other in the form of a ſpark.

On the ſpontaneous electricity of this inſtrument.

I have made many experiments with this inſtrument relative to its ſpontaneous electricity, and I find, as a ſingle multiplier (that is when an electrometer is connected with C) it has no effect on the moſt delicate gold leaf electrometer I could make; but as a double multiplier (that is when an electrometer is connected with E) there is ſome ſmall effect if ſome electricity had been communicated to it within an hour or two, although it may have been diſcharged by touching each of the plates with a metal point (which I find is the moſt effectual way of diſcharging ſmall portions of electricity,) but if it has ſtood three or four hours after being diſcharged, it will not give any ſigns of electricity. When uſed as a doubler it always becomes electrified with between eight and ſixteen motions of the lever,

On the ſpontaneous electricity of this inſtrument.

even though it has not been uſed for two or three months.— But if it has been uſed within two or three hours, the effect will take place with fewer motions of the lever, and if it has been uſed within a few minutes, two or three motions will be more than ſufficient to aſcertain the quality of the electricity. It is to be obſerved that the inſtrument was always diſcharged (by the metal point as before obſerved) between each trial.

In the courſe of making the experiments on the ſpontaneous electricity of this inſtrument I found that it was always poſitive if the inſtrument had not been uſed for two or three days, whatever electricity it was laſt charged with. But the time it muſt ſtand unuſed after it has been diſcharged, to produce this effect, depends a great deal on the weather; if the air is very humid, twenty-four hours is quite ſufficient, but if it is very dry, it will require four or five days.

After I had obſerved that after the inſtrument was charged with poſitive electricity its ſpontaneous electricity was always poſitive; and that after it was charged with negative electricity, its ſpontaneous electricity was negative only within a certain time after it had been diſcharged, and then became poſitive; and alſo that it required a greater number of motions of the lever to produce a certain effect on the electrometer with negative electricity, the longer it ſtood after it had been charged with negative electricity, and that when it became poſitive, the longer it ſtood the leſs number of motions of the lever it required to effect the electrometer with poſitive electricity to a certain degree, and this within ſome certain limits. I was at a loſs how to account for this change. However, after ſome conſideration, I began to ſuſpect that the plates (although all of the ſame metal, copper) had each a property peculiar to itſelf, of acquiring a certain ſmall charge of one kind of electricity in preference to the other; and that if they were left to themſelves they would naturally do ſo. I therefore began a ſet of experiments to aſcertain the probability of this ſuppoſition. Firſt, directly after the inſtrument had been uſed for negative electricity, I diſcharged it by touching each of the plates with a metal point, which I held in contact with each plate for two or three ſeconds. An electrometer was then connected with the plate A, and while it was in this ſituation, it was made to diverge with poſitive electricity;* it was then

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* The electrometer was made to diverge by bringing an excited glaſs or ſealing wax near it.

On the ſpontaneous electricity of this inſtrument.

diſcharged by a ſlight touch with the finger, the lever was then moved backwards and forwards about fourteen times, and the electrometer diverged with negative electricity. This was repeated ſeveral times with the ſame effect.

The lever was then put in ſuch a poſition that the plates could not be ſuppoſed to act as condenſers, one pair more than another, and the whole was left untouched for twenty-four hours. It was then tried, and with twenty motions of the lever the electrometer diverged with poſitive electricity.

It was then left untouched for eighteen hours more, and on trial the electrometer diverged with poſitive electricity. The inſtrument was next diſcharged with the metal point as before, and the electrometer made to diverge with negative electricity, which was diſcharged by a ſlight touch of the finger. The lever was now worked, and with nine motions of the electrometer diverged with poſitive electricity. It was then diſcharged and left untouched for ſixteen hours, after which it diverged with poſitive electricity after a few motions of the lever. It was then left untouched for five days, after that time it gave poſitive electricity when the lever was worked. Theſe experiments I have repeated a great many times with the ſame effect.

As it appeared from the foregoing experiments that the reſiduum on the electrometer was inſufficient to overcome the effect of the reſiduum on the plates, I was induced to charge the whole inſtrument with the effect of the electricity, I would wiſh to communicate to it, and this I did by making the electrometer diverge with the deſired electricity, and while it was divergent I began working the lever, to communicate the effect to all the plates. I charged the inſtrument with negative electricity firſt, and diſcharged it with the metal point. The lever was then worked, and in eight motions the electrometer diverged with negative electricity. It was then left untouched for five days, when eighteen motions of the lever made the electrometer diverge with poſitive electricity. This has been often repeated with the ſame effect.

The inſtrument was next charged with poſitive electricity, and diſcharged as before, after which, ſix motions of the lever made the electrometer diverge with poſitive electricity. It was left untouched for twenty-four hours, when the electrometer

On the ſpontaneous electricity of this inſtrument.

trometer diverged with poſitive electricity with twelve motions of the lever, and ſo it did after it had ſtood untouched for ſix days.

The probability of the foregoing ſuppoſition concerning the property of the plates acquiring a charge of one electricity in preference to the other, was more ſtrengthened by the following experiment, which has been repeated ſeveral times. The inſtrument was charged with negative electricity, and then left without diſcharging for twenty-four hours, when ſixteen motions of the lever made the electrometer diverge with poſitive electricity.

I make no doubt if the plates were of different metals this effect would be more ſtriking, and that in ſome caſes we ſhould have the contrary electricity. I intend to conſtruct one of theſe inſtruments in ſuch a way that plates of any metal may be put on, and their effect tried. I think this a ſubject well worth purſuing as it may throw ſome light on ſome of the phenomena of electricity, which have their cauſes at preſent buried in obſcurity.

V.

Note reſpecting the Suſpenſion of Zinc in Hidrogen, and the conſequent Ignition and Fuſion of Platina Wire. By Amicus.

To Mr. NICHOLSON.

Dear Sir,

Ignition and fuſion of platina in Cuthbertſon's gazometer,

I EMPLOY a *gaſometer* of a much more ſimple conſtruction and much leſs expence than Lavoiſier's, which that ſkilful and intelligent artiſt, *Cuthbertſon,* invented at my entreaty, about ten years ago. It is deſcribed in your valuable Journal (quarto ſeries, vol. II. p. 235.) The braſs rod or the thick wire, which conveys the electric fire through the upper receiver, has about half an inch of platina wire appended, to hang juſt over the aperture through which the hydrogen gas riſes at the bottom in the braſs work. Zinc of any other ſort of metal would either be melted or readily oxided. But the other day, I was much ſurprized to ſee the platina wire, on becoming as uſual *red hot* from the flame of the hydrogen gas, melt into globules as readily as copper or braſs wire would have done. I ſoon perceived from the *colour* of the flame of the hydrogen and the depoſit of *white clouds* on the inſide of the glaſs, that the rapid ſolution of the finely powdered zinc uſed to afford the gas, that this metal was actually ſuſpended or diſſolved in the gas, and hence the zinc acting to the platina fuſed the latter. It may be perhaps uſeful to know this fact, which occurred during the public lecture.

—cauſed by the volatilization of zinc, &c.

Your's, as uſual,

AMICUS.

July 2, 1804.

VI.

Deſcription of a Blow-Pipe acting by the Preſſure of Water, by the Abbé Melograni. In a Letter from Mr. G. B. GREENOUGH.

To Mr. NICHOLSON.

SIR,

No. 15, *Parliament-Street,*
Auguſt 13, 1804.

PERMIT me to make you acquainted with the principle of an inſtrument invented by the Abbé Melograni, and uſed by him at the Royal Mineralogical Collection at Naples, as a ſubſtitute for the blow-pipe. I regret that I cannot ſend you an accurate account of its conſtruction, as I made no ſketch at the time, and ſpeak only from recollection.

Blow-pipe ſupplied by two veſſels alternately changing their poſitions.

Two hollow glaſs globes of convenient ſize were connected together by two braſs tubes laid along-ſide each other, one of them having a valve or ſtop cock, I am not ſure which, at each end, and a ſide tube of the ſame metal going off from the middle at right angles. The frame was attached to the vertical tubes, ſo as to allow the globes to circumvolve each other. Let the lower globe be half filled with water and inverted; Then the water in A (*Fig.* 2. *Plate* IV.) running into B through the tube C, will force a conſtant ſtream of air into the tube D, and thence, the upper orifice being cloſed, into the ſide-tube E, at the mouth of which the candle is placed. When the water is nearly run out invert the globes.

If the principle of this inſtrument ſhall appear to you deſerving of the public attention, it would give me pleaſure to ſee it ſubmitted to the readers of your excellent Journal. I have the honour to ſubſcribe myſelf,

Sir,

Your obedient humble ſervant,

G. B. Greenough.

VII.

Outlines of the Mineralogical Syſtems of Romé de Liſle and the Abbé Hauy; with Obſervations. By the Abbé Buée. Communicated by the Hon. Robert Clifford.

To Mr. NICHOLSON.

SIR,

Outlines of the mineralogical ſyſtems of Romé de Liſle, and the Abbé Hauy.

WHEN the public mind appears at any time to direct itſelf towards a particular ſcience (as it does at preſent towards mineralogy,) no perſon can feel the neceſſity of removing any erroneous opinions relating to that ſcience, more forcibly than a gentleman who dedicates his time, as you do, Sir, to propagate correct ſcience by means of your Journal. Actuated by the ſame ſentiments, may I requeſt the inſertion of the encloſed paper, whoſe object is to remove ſeveral erroneous opinions relating to the mineralogical ſyſtems of Romé de L'Iſle, and of the Abbé Hauy. It was written by the Abbé Buée in French for a friend of his, but on its being communicated to me by the author, I requeſted leave to ſend an Engliſh tranſlation of it to you, thinking it might be acceptable to thoſe who are fond of philoſophical purſuits. Should your opinion coincide with mine, you will certainly afford it a place in one of your enſuing numbers.

And I remain, Sir,

Your very humble ſervant,

Robert Clifford.

Welbeck-Street, July 13, 1804.

A Letter

A Letter from the Abbé Buée to Mr. —, on Mr. Romé de L'Iſle's and the Abbé Hauy's Theories of Cryſtallography.

Sir,

Parallel between the theories of Romé de l'Iſle and Hauy.

IN conſequence of your requeſt, I ſend you the parallel of the two Theories of Cryſtallography, which ſeem to divide mineralogiſts in this country, thoſe of Mr. Romé de L'Iſle and of the Abbé Hauy. You are perfectly acquainted with the former theory, but nearly a ſtranger to the latter. Having lived for ſix and thirty years in habits of intimacy with the Abbé, I dwell with pleaſure on his works, and will do my utmoſt to ſatisfy your curioſity.

De l'Iſle firſt exhibited order in cryſtallography.

To Mr. de L'Iſle is due the merit of having called the attention of naturaliſts to that neglected branch of mineralogy, cryſtallography; of having diſcovered that that branch, though neglected, was perhaps the moſt intereſting part of mineralogy, and the only part which could raiſe it to the dignity of a correct ſcience; in ſhort, of having diſcovered order, by numerous obſervations as ingenious as new, there, where a Cronſtet, a Bergman, a Buffon, or a Kirwan could perceive nothing but confuſion; and thus ſeemed to reſcue nature from the charge of caprice, almoſt imputed to it becauſe great mineralogiſts had neglected to ſtudy its unerring laws.

Hauy firſt explained the laws of cryſtallization.

It was excluſively reſerved to the Abbé Hauy to point out, to explain, and apply thoſe laws. He demonſtrated where De L'Iſle affirmed. He diſcovered thoſe *hidden facts*, which he has ſince ſhewn to be the mathematical conſequences of facts obſerved by De L'Iſle. If the latter furniſhed a part of the materials, the Abbé has augmented and employed them. The diſcoveries of theſe two writers force me to ſubdivide cryſtallography into two diſtinct parts; deſcriptive and philoſophical; and under theſe two heads I will rapidly deſcribe the labours of each author.

Deſcriptive cryſtallography. De l'Iſle's tables.

Deſcriptive.—The moſt important part of Mr. de L'Iſle's work conſiſts in his cryſtallographical tables. In each of theſe tables (ſeven in number) he deſcribes one of the principal forms aſſumed by cryſtals, and then delineates the different modifications of which that form is ſuſceptible, by means of different troncations *(troncature)* as he calls them.

Modifications of cryſtals deſcribed by troncations. Inſtance the cube.

For elucidation, let us take a cube, the primitive form of the ſecond table. A cube, it is known, has ſix faces, eight ſolid angles, and twelve edges. If the cube be truncated in a parallel to one of its faces, a rectangled parallelipipedon will be produced, and the equality of the faces will be deſtroyed.

If the eight ſolid angles of the cube be ſtruck off, eight new faces will replace the eight ſolid angles, and in place of ſix ſides we ſhall have fourteen. If the twelve edges be taken off, twelve new faces will ſucceed the ſtrait lines, and the ſolid will have eighteen ſides. Such are De L'Iſle's *ſimple troncations.* They may be then combined with each other, and made more or leſs deep; hence an immenſe variety of new figures. But theſe new forms again may be truncated in the directions either of their faces, ſolid angles, or edges, and theſe new troncations more or leſs deep, called by De L'Iſle *ſur-troncatures*, may alſo be combined with each other. Here the forms muſt multiply to infinity, and their boundleſs numbers will ſoon bury the primitive cube in oblivion.

It muſt not be ſuppoſed that nature has furniſhed us with this infinite ſeries of forms; indeed Mr. De L'Iſle in his tables has only mentioned thoſe he had obſerved, with ſome few additional ſuppoſitious figures, of which ſeveral have been ſince diſcovered to exiſt.

Account of De l'Iſle's ſeven tables of cryſtals.

This ingenious naturaliſt has given us, as I have already ſaid, ſeven cryſtallographical tables. In the 1ſt he deſcribes the tetrahedron and its modifications; in the 2d the cube; in the 3d the rectangular octahedron; in the 4th the rhomboidal parallelipipedon; in the 5th the rhomboidal octahedron; in the 6th the dodecahedron, with triangular faces, and to each are ſubjoined their reſpective modifications. The object of the 7th table is to point out certain modifications of the octahedron and parallelipipedon, whether rectangular or rhomboidal. Plates accompany each table, where the figures are drawn, and in the obſervations and notes on them are to be found the meaſures of the principal angles.

They contain general ſolids; which are ſubſequently applied to the cryſtals of nature, viz. *ſalts, ſtones, and metals.*

Theſe cryſtallographical tables exhibit only general repreſentations of ſolids, which Mr. de L'Iſle in the courſe of his work applies to the different cryſtals which had already been diſcovered, and fallen within his obſervation. His work conſiſts of three parts. In the firſt he treats of ſaline cryſtals; in the ſecond of ſtoney *(pierreux)* cryſtals, and in the third of metallic cryſtals. Thoſe of the firſt claſs are artificial, thoſe of the two latter claſſes are natural cryſtals, and are ſubdivided into genera, ſpecies, and varieties.

His method of deſcription.

When treating of a ſpecies or of a variety, he refers his reader to the table where the figure of that ſpecies or variety is to be found, and he then enumerates every thing relating to minerals aſſuming that cryſtalline form: But I cannot terminate this ſketch better than by the following extract from the Abbé Hauy's treatiſe on mineralogy.

Quotation exhibiting a ſummary of De l'Iſle's labours.

"In ſhort Romé de L'Iſle reduced the ſtudy of cryſtallography to principles more exact and more conſiſtent with obſervation. He claſſed together, as much as he was able, cryſtals of the ſame nature. From among the different forms belonging to each ſpecies, he ſelected one which appeared to him to be the moſt proper, on account of its ſimplicity, for the primitive form, and then ſuppoſing it to be truncated in different manners, he deduced the other forms, and eſtabliſhed a certain gradation or ſeries of paſſages from the primitive form to that of polyëdrons which would ſcarcely appear to have any connection with it. To the deſcriptions and figures which he gave of the cryſtalline forms, he added the mechanical meaſurement of the principal angles, and he ſhewed (a moſt eſſential point) that theſe angles were conſtantly the ſame in each variety. In a word, his cryſtallography is the fruit of immenſe labour by its extent; almoſt entirely new in its object, and of great value for its utility." (Vol I. page 17.)

The mineralogy of Hauy is not merely deſcriptive, but phyſical, chemical and geometrical.

The Abbé Hauy in his treatiſe on mineralogy embraces a far greater extent than Mr. de L'Iſle. His mineralogy is not only deſcriptive, but it is phyſical, chymical and geometrical. In the perſuaſion that a mineral cannot be well deſcribed, nor even in many caſes recognized, unleſs its phyſical, chemical, and geometrical characters are clearly laid down, the Abbé never omits any one of thoſe characters when aſcertained, and expoſes with the moſt ſcrupulous exactneſs every thing relating to them that obſervation has authenticated. He has beſtowed particular attention to the electrical and magnetic phenomena, and has enriched the ſcience with a multitude of new and curious obſervations. He attentively examined the property of double refraction, which ſeveral tranſparent minerals enjoy; and here again he has extended the boundaries of ſcience. A few minerals were known to poſſeſs this property, and the Abbé has diſcovered it in ſeveral where it had never been ſurmiſed.

Electricity. Magnetiſm.

Double refraction.

Hauy has excluded none of the habitudes,

When we conſider that writers on mineralogy have hitherto grounded their ſyſtems *excluſively*, ſome on the exterior characters, or properties of minerals.

racters, others on the chemical properties of minerals, and that the Abbé really has, purſuant to his plan, (ſee in the beginning of the volume of plates, *La diſtribution methodique des mineraux, par claſſes, ordres, Genres et eſpeces*, the methodical diſtribution of minerals into claſſes, orders, genera, and ſpecies) united all that has hitherto been diſcovered on mineralogy, without falling into that confuſion which has ever been imputed to other mineralogical writers, we are almoſt aſtoniſhed at his ſucceſs. "To claſs minerals, to furniſh the means of diſcovering to which claſs, genus, and ſpecies a mineral under examination belongs," are the two great problems which the Abbé Hauy propoſes for ſolution.

Claſſification, chemical: inveſtigation by external characters.

He ſolves the firſt in following Bergman's method (founded on chemical properties) much improved by the immenſe progreſs which chemical analyſis has made ſince the days of that great chemiſt. In the ſolution of the ſecond he follows Werner's method (grounded on exterior characters) but corroborated by a multitude of new experiments, eaſily made and brought to a ſurprizing degree of correctneſs by the Abbé's own labours on the forms of cryſtals. But I perceive that the immenſity of matter contained in this treatiſe is leading me from that point which I had particularly in view, I mean cryſtallography.

Hauy's deſcription of cryſtals, 1. by delineation.

In the deſcription of cryſtals the Abbé employs three different means. 1ſt He draws their figure; he does not give cryſtallographical tables as Romé de L'Iſle, which are only general properties, but draws ſeparately each ſpecies and variety. Every form given in the plates has been examined by himſelf; he has calculated every angle, and neverthleſs his plates contain one third more figures than De L'Iſles tables.

2. By ſymbolic ſigns deſignating the laws of their production.

2d He makes uſe of ſymbolic ſigns, than which nothing can be more ſimple, and were invented not to recall the form of the cryſtal to the mind, but the laws by which it has been produced. Yet I have met with perſons, who were ſo accuſtomed to theſe ſigns, that at firſt ſight of them they could immediately figure to themſelves the form of the correſponding cryſtals. Theſe ſigns can alſo be ſpoken, and much circumlocution in conſequence avoided.

3. By nomenclature.

3d A ſignificant nomenclature ſubdivided into general and particular. The general is for the *minerals*, and compriſes only ſubſtantives; the particular for the *cryſtals*, and is entirely compoſed of adjectives. He ſtudiouſly avoided introducing new names, and nevertheleſs has been obliged to introduce many, where new ſubſtances, names capable of giving falſe impreſſions, or others void of ſignification and unſupported by long uſage, required it. He then ſubſtituted names taken from the Greek; a language, he ſays, that eminently enjoys the faculty of combining ſeveral words together, ſo as to form one repreſenting conciſely the object to be named.—The adjectives uſed in the nomenclature of the cryſtals alſo allude to ſome remarkable circumſtance of the cryſtalline form.

Philoſophical cryſtallography.

I ſhall now proceed to philoſophical cryſtallography, which might be called the philoſophy of mineralogy. It does not conſiſt in ſearching for the primary cauſes of phenomena, nothing can be leſs philoſophical than ſuch a reſearch; primary cauſes will ever be beyond the reach of the human mind! The immortal Newton was the firſt to point out to us by the method followed in his admirable book of the Principia, that the only true philoſophical way of treating a phyſical ſcience, or of explaining a natural fact, was to demonſtrate that it was the mathematical conſequence of a general law, grounded on an aggregate of facts already obſerved and capable of correct calculation. If any one of theſe conditions are wanting, we immediately launch out into hypotheſis, explanations become vague, and however much we may be perſuaded of the truth of our aſſertions, we can acquire no certainty.

The true method of explaining physics is to ſhew that the facts are mathematically deducible from a general law.

Hypotheſis.

The method of Rome de l'Iſle ſhewn to be deficient in correctneſs.

Let us apply theſe principles to our two writers. De L'Iſle, in declaring that the various forms obſerved in cryſtals of the ſame ſubſtance are only modifications of one conſtant primitive form, certainly announced a moſt important truth. It was a flaſh of genius; but in a philoſophical enquiry, to prove it and not ſimply to ſay it, was the neceſſary ſtep. On the firſt inſpection of his cryſtallographical tables, a ſtudent is tempted to think that important truth demonſtrated; but on a cloſer examination, the impreſſion is done away. The ſame order pervades every table. By ſlight paſſages the ſtudent is led from the ſimpler to the more compound forms, and after every paſſage, is tempted to ſay; this can only be a modification of the primitive; then when the real cryſtals, and the figures of the tables are compared together, and all thoſe of the ſame ſpecies (with a very few exceptions) are found in the ſame table, how eaſy it is to perſuade ourſelves that nature *muſt* operate by ſimilar paſſages when producing the various forms of cryſtals; and the primitive of the table before us *muſt* be the primitive of the cryſtal under examination. In a word it is the moſt ſimple form; and firſt impreſſions greatly ſtrengthen the illuſion. If perſuaſion was the ſole object of philoſophy, De L'Iſle would have been a powerful philoſopher; but philoſophy muſt convince, demonſtrate, and wreſt conſent, however violently oppoſed. An enemy muſt not therefore be able to make uſe of the ſame arms, or adduce the ſame proofs to eſtabliſh a contrary opinion. Nevertheleſs ſuch would be caſe with Mr. De L'Iſle's tables and the application of them: For it is an inconteſtable fact, that by a ſeries of arbitrary troncations we may paſs inſenſibly from any given form to any other. Grounded on this principle, and ſeconded by Mr. de L'Iſle's ingenuity, any form may become primitive, and any other deduced from it. Now as the combinations are infinite, a multitude of tables may be conſtructed, forms of the ſame ſpecies may be diſperſed in different tables: the moſt ſimple of each table will be the primitive, therefore forms of the ſame ſpecies will have different primitives. But when by the ſame principle both ſides of the queſtion can be proved, nothing is proved.

and may be employed to eſtabliſh falſhood.

On the moſt ſimple form.

To ſay the moſt ſimple form muſt be the primitive is an illuſion, for we know not what is the moſt ſimple for nature. With our feeble organs and confined ſenſes we can form no judgment of *ſimple*, when the operations of nature are in queſtion. Nature embraces the entire univerſe, her laws are ſimple; but the combinations made according to thoſe laws are unbounded, therefore complicated.

The ſyſtem of troncations is the moſt obvious.

Let us not forget, however, that the idea of troncations, and the idea of taking the moſt ſimple form for the primitive, are ſo natural that they muſt have been the firſt to preſent themſelves to the man who was opening the carreer: " Often, ſays the Abbé Hauy, (vol. I. page 14) a more compound form only differs from a more ſimple one by certain little faces which may be produced by ſections, either at the ſolid angles, or on the edges of the ſimpler form," and in a note he ſays, " this was the obſervation which gave the celebrated Romé de L'Iſle the idea of his ſyſtem of troncations, that he might ſucceſſively deduce from each other the different varieties of cryſtalline forms aſſumed by the ſame ſubſtance."

Mr.

De l'Iſle's propoſed axiom.

Mr. De L'Iſle terminates the introduction to his work by certain axioms, as he ſtyles them, the 2d and 16th are as follow:

" II. Every angular polyhedron, or every cryſtallized ſubſtance is a SALT in the moſt extended acceptation of that term."

" XVI. Every ſaline ſubſtance whoſe conſtituent parts are perfectly ſaturated and combined affects the *cubic* form, or its inverſe the *octahedron;* whereas the ſalts which are not neuter, or whoſe conſtituent parts are not exactly combined, affect either the *priſmatic* or the *rhomboidal* forms."

I need ſcarcely obſerve that, to treat theſe axioms only as doubtful, would be treating them kindly. The other axioms are matters of fact, from which he draws no conſequence, and indeed it would have been difficult for him to have drawn any.

Hauy's exhibition of the primitive form in cryſtals,

by analyſis and ſyntheſis.

The Abbé Hauy does not undertake to prove generally, that among the different cryſtalline forms of the ſame ſubſtance, one of them is the primitive; but he produces that primitive form from each cryſtal, which is always ſimilar in ſimilar ſubſtances. He demonſtrates it analytically and ſynthetically; by an analyſis which might be called *mineralogical analyſis,* and pointed out by nature herſelf: By a ſyntheſis hitherto the property of mathematicians, but here ſupported by the general laws which his analyſis has revealed to him. The conſtant accord found between this ſyntheſis, and daily obſervation is a proof of the exactneſs of his method.

Fundamental facts. 1. Cryſtals may be cleft in certain directions only; 2. their boundaries are right-lined or plane.

Two facts were the foundation of his theory: 1. In all times jewellers and lapidaries have remarked that ſtones are more eaſily cut in ſome certain directions than in others. 2. Whoever has been in the habit of ſeeing natural cryſtals muſt have obſerved, that when their forms are well determined, they are always terminated by plane ſurfaces. Thus, ſays the Abbé, "thoſe ſoft outlines, and that roundneſs ſo frequent in the animal and the vegetable kingdom, where they are inherent to the organization, and contribute even to the elegance of the forms, indicate on the contrary in minerals a want of perfection. The characteriſtic of true beauty in minerals is the ſtrait line, and it was with truth that Romé de L'Iſle declared *that line* to be the peculiar property of the mineral kingdom." The firſt fact ſuggeſted the mineralogical analyſis, and the ſecond furniſhed him with the laws on which he grounded his ſyntheſis.

Enquiries on the firſt Fact.

On the analyſis or ſubdiviſion of cryſtals into regularly formed portions by fiſſure.

1. All cryſtals that can be ſplit by means of inſtruments, offer to the view, if ſplit in certain directions, plane and ſmooth ſurfaces. If divided in other directions, the fracture is rugged. I uſe the word *ſplit*, and not *ſawed* or *cut*, as the ſections of the cryſtal are not to be obtained by ſlow and continued efforts, but by ſudden ſhocks. Patience, dexterity, and habit enabled the Abbé to ſplit a great number of cryſtals; in all he diſcovered plane ſmooth ſurfaces when ſplit in certain directions, but when in other directions, the fracture was always rugged and irregular. I requeſt, Sir, your attention to this important fact, it is fundamental, and the more important, as ſeveral perſons of much general information have neglected to attend to it, and, in conſequence, have ſuppoſed the whole of this theory to be grounded on hypotheſis. It would be equally erroneous to confound theſe ſections of cryſtals with De L'Iſle's troncations. The latter indeed warns his readers, that by the word *troncations* he wiſhes only to figure the appearance of the cryſtal examined. They are not therefore real, but only a means of warping the imagination to the exterior form of the cryſtal, and are by their nature only deſcriptive. The Abbé Hauy's ſections are real, and are pointed out to the obſerver by the interior ſtructure of the cryſtals; they are experimental.

It is *experimental*, whereas the troncations of De l'Iſle were *hypothetical*.

The inclinations of the faces are conſtant.

2. The plane ſmooth ſurfaces obtained by the above method are reſpectively parallel to 3, 4 or 6 planes. The mutual inclination of theſe planes to each other are conſtant in cryſtals of the ſame ſubſtance, whatever may be the exterior form of the cryſtal. Native antimony, phoſphate of lead, and quartz ſeem to ſhew an appearance of more than ſix planes, and the Abbé Hauy leans to the opinion of only five planes in ſome caſes; but as theſe are exceptions to the general rule, and would only tend to complicate this ſtatement, I ſhall take no further notice of them.

Explanation of the figures and formation of cryſtals by the method of Hauy.

Let us ſuppoſe the ſmooth ſurfaces to be only parallel to three planes, or in other words, that the ſubſtance will only ſplit in three directions, in that caſe the ſections can only produce a parallelipipedon, whoſe nature is determined by the mutual inclination of the planes to each other. If the planes are perpendicular, it will be rectangular, &c.

We next ſuppoſe the ſmooth ſurfaces to be parallel to four planes. Here a diſtinction ariſes, whether three of theſe planes

Explanation of the figures and formation of cryſtals by the method of Hauy.

have a common interſection or not; and it muſt be remembered that if the four planes have a common interſection, no ſolid can be produced, as they can neither bound nor include a ſpace. If therefore three of the four planes have a common interſection, the ſplittings will produce either one hexahedral priſm, or three parallelipipedons, which will be ſimilar or diſſimilar, according to the ſimilarity or diſſimilarity of inclination of the planes, or one triangular priſm. On the contrary, if the four planes only interſect each other two and two, there will be produced either one octahedron, or four parallelipipedons, or one tetrahedron.

Laſtly, let us ſuppoſe the ſmooth ſurfaces to be parallel to ſix planes; then there ariſe an immenſe number of caſes.—But we will for the preſent confine ourſelves to the only caſe that has hitherto been obſerved in nature: Where the interſection of the planes is two and two, then we obtain either, 1. dodecahedron with pentagonal, quadrilateral or triangular ſides, according to the ſections made, or fifteen octahedrons, or twenty parallelipipedons, or fifteen tetrahedrons. It may be proper to obſerve here that though the ſections parallel to the ſix planes may be clearly indicated, nevertheleſs it rarely happens they can all be executed, but it will ſuffice for the purpoſes of geometry that they be clearly indicated to render the conſequences drawn from them mathematically correct.

Having laid down theſe premiſes, let us proceed to the diſſection of a cryſtal of carbonate of lime (the ſpath calcaire of De L'Iſle) whoſe primitive form is a rhomboid or a parallelipipedon bounded by rhombs. Hitherto ſections have only been obtained in the three directions parallel to its ſides. If theſe ſections be directed ſo as to always paſs through the center of two oppoſite ſides, they will produce eight rhomboids equal to each other, and ſimilar to the original one. The ſame operation may be repeated on each of theſe eight rhomboids, and continued ſo long as the ſubſtance remains carbonate of lime, that is to ſay, to be a combination of 55 parts of lime, 34 of carbonic acid, and 11 parts of water of cryſtallization:—(ſee Bergman.) But this diviſion of the cryſtals into ſimilar ſolids has a term, beyond which we ſhould come to the ſmalleſt particles of the body, which could not be divided without chemical decompoſition; that is to ſay, without an alteration

Explanation of the figures and formation of cryſtals by the method of Hauy.

in the proportions of lime, carbonic acid, and water. Theſe laſt particles which are ſtill rhomboids, are what the Abbé Hauy calls the *integrant particles* of the carbonate of lime. In the ſuppoſition therefore that a rhomboid of this ſubſtance can only be divided in three directions, by ſections parallel to the ſides, it is evident that the integrant particles muſt be ſimilar rhomboids.

If a cryſtal can be divided by ſections in more directions than three, what will be the form of the integrant particles? For example, in the phoſphate of lime (the chryſolite of De L'Iſle) where the ſections are parallel to four planes, three of which have a common interſection. According to what has been ſaid above, theſe ſections can produce either one hexahedral priſm, or three parallelipipedons, or one triangular priſm. It is evident that by carrying the diviſion, according to thoſe ſections, to its greateſt length, either the laſt hexahedral priſm, or the laſt three parallelipipedons, or the laſt triangular priſm, will be produced. Are theſe laſt ſolids the integrant particles; are each of them ſo; or is there only one of them entitled to that denomination; and if only one, which of them? My anſwer is, only one of them; and that one, the triangular priſm, which may be proved thus.

It cannot be denied that the integrant particle is that little ſolid which contains the leaſt poſſible quantity of the body, without affecting the chemical compoſition of the ſubſtance. This granted, let us ſuppoſe the hexahedral priſm to be the integrant particle. In that ſuppoſition the laſt triangular priſm muſt contain the laſt hexahedral priſm, and is equal to the latter more three little triangular priſms, or in other words to nine ſimilar triangular priſms, while the hexahedral priſm only contains ſix. But the laſt triangular priſm and the laſt hexahedral priſm each contain an exact proportion, and therefore a ſimilar proportion of chemical component parts; therefore their difference alſo contain an exact proportion; but it is impoſſible to conceive how their differences can contain the exact proportion, unleſs each of the three little triangular priſms alſo contain it, they muſt therefore contain it, and each of them muſt be an integrant particle; therefore the hexahedral priſm cannot be one; neither can the parallelipipedons be integrant particles, as the ſame arguments will ſtand good againſt them which have been applied to the hexahedral priſm; there-

fore

Explanation of the figures and formation of cryſtals by the method of Hauy.

fore the triangular priſm muſt be the integrant particle:* "The forms of the integrant particles, ſays the Abbé (vol. I. page 30) may be reduced to three, the tetrahedron or the moſt ſimple of pyramids, the triangular priſm or the moſt ſimple of priſms, and the parallelipipedon or the moſt ſimple of ſolids, having parallel ſides two and two, and as four ſides are neceſſary to circumſcribe a ſpace, it is evident that the above three forms in which the number of ſides are ſucceſſively four, five and ſix, are again in this point of view the moſt ſimple poſſible."

The phoſphate of lime or chryſolite is a ſubſtance that has given riſe to much curious anecdote. It ſhews in what a ſtate Abbé Hauy found the mineralogical nomenclature, and points out the accuracy of his analytical method. Achard, a chemiſt at Berlin had analyſed the chryſolite, and publiſhed that it contained, of ſilex 15 parts, alumina 64, lime 17, and of iron one. This ſtartled the celebrated Vauquelin, who had ſeen Klaproth's analyſis of the chryſolite (the apatite of Werner) containing of lime 55 parts, and of phoſphoric acid 45, (probably the water of cryſtallization is added to the acid.) A Frenchman of the name of Launoy ſent a quantity of this ſubſtance to Paris, ſome of it was purchaſed by the *Ecole des Mines*, and Vauquelin was deſired to analyſe it. The latter ſoon ſuſpected Achard had been miſled by the name, and had not obtained the proper ſubſtance, a miſtake the more eaſily made as, ſays Vauquelin, " the name of chryſolite was given to a great variety of ſtones, ſuch as the *peridot*, the *chryſoberil*, the *olivine*, (ſince found to be the ſame as the *peridot)*, and in general to *ſtones* having a *yellow colour*."

He ſoon diſcovered the chryſolite ſent from Spain contained lime and phoſphoric acid. " I had no ſooner made this diſcovery, ſays he, than I enquired of Abbé Hauy whether he had compared the integrant particles of the chryſolite with thoſe of the apatite or cryſtallized phoſphate of lime. He anſwered me that he had not made the compariſon, but that he would get his papers on primitive forms, (this was four years before the publication of his work) ſee what notes he had

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* Therefore the ſections producing the hexahedral priſm cannot lead to the integrant particle: therefore all ſections, though perfectly practicable in cryſtals, will not lead to the integrant particle.

made

Explanation of the figures and formation of cryſtals by the method of Hauy.

made on each of thoſe ſubſtances, and immediately compare them together; when with pleaſure he found that there was not the leaſt variation between them. Thus had the Abbé Hauy diſcovered by the help of geometry alone, that which was confirmed by chemical analyſis, and this ſatisfactory accord between two ſciences apparently ſo diſtant from each other, while ſecuring each others ſteps, ſerves alſo to ſhew the certainty of the principles on which they are grounded."

Journal des Mines, xxxvii. page 21.

A more ſingular anecdote is what took place with reſpect to the emerald and the beril. Vauquelin had analyſed the emerald of Prerou, and read the reſult of it, at a ſitting of the *Ecole des Mines*, which is preſerved in the Journal, No. XXXVIII. page 96: viz. of ſilex 64, of alumina 29, oxide of chroma 3, of lime one, and of volatile ſubſtance 2; I have neglected decimals. Soon after he diſcovered a new earth, which he called the Glucine, and gives the following account of it to the National Inſtitute: "The Abbé Hauy having obſerved a perfect conformity between the ſtructure, the hardneſs, and the weight of the beril and the emerald, preſſed me a few months back to make an analyſis of theſe two ſubſtances, to know whether they contained the ſame principles, and in ſimilar proportions. In the reſult, the fact that will moſt intereſt the Inſtitute is the diſcovery of a new earth, &c. &c." *Annales de Chimie*, vol. XXVI. p. 157.

I am certain, Sir, that it will give you no leſs pleaſure to learn that Vauquelin, in conſequence of this diſcovery, made an *addition to the paper* read at the *Ecoles des Mines*, beginning thus, "Since the reading of the above paper, having diſcovered a new earth in the beril, and as this ſtone, according to the obſervations made by Hauy, contained ſubſtances ſimilar to the emerald, I have in that point of view made a new analyſis of this latter ſtone." And the former analyſis was immediately corrected, and the 29 parts of alumina became 16 parts of alumina and 13 *of a new earth*. I hope, Sir, it is not too much to ſay that, on this occaſion a new earth was diſcovered, if not by, at leaſt in conſequence of, a geometrical analyſis.

But to return to our ſubject, the Abbé makes a diſtinction between the *integrant particle* and the *primitive form*. The former, as I have ſaid, is that laſt particle, which preſerving an exact proportion of the component parts, contains the *leaſt*

number

Explanation of the figures and formation of cryſtals by the method of Hauy.

number of thoſe parts; it is the laſt term of mineralogical analyſis. The primitive form, on the contrary, is its firſt reſult, and retaining the exact proportion of the component parts, contains the *greateſt number* of thoſe parts. It is eaſy to ſee that in the caſe above mentioned of the phoſphate of lime, the hexahedral priſm will be the primitive form, preciſely for the reaſons adduced to ſhew that it is not the integrant particle. Though the Abbé does not decidedly define the integrant particle as containing the *minimum* of ſpace under the *maximum* of ſurface, and the primitive form as containing the *maximum* of ſpace under the *minimum* of ſurface,* nevertheleſs he makes a remark that authoriſes the above definitions (which, Sir, you will obſerve are mine, leſt any fault be found with them.) He ſays the dodecahedron with rhomboidal ſides, which is the primitive form of the garnet (grenat) contains the *maximum* of ſpace under the *minimum* of ſurface; and if it be cut into two equal and ſimilar parts, it will preſent the ſame form as the bottom of the cell of the honey-comb, which has the ſimilar property.

An objection might be taken on the *cuivre pyriteux*, and the *cuivre gris*, or the yellow and grey copper ore of Kirwan, the Abbé mentioning the regular tetrahedron as their primitive form, and not the octahedron as in other caſes. The reaſon may be, that all the cryſtalline forms of their ſubſtances which he deſcribes are ſlight modifications of the regular tetrahedron.

"The primitive forms hitherto obſerved, ſays the Abbé Hauy, are reduced to ſix.—The parallelipipedon, the octahedron, the tetrahedron, the regular hexahedral priſm, the dodecahedron bounded by rhombs all equal and ſimilar, and the dodecahedron with triangular ſides, formed by two right pyramids united baſe to baſe."

He alſo makes a diſtinction between integrant particles and *ſubſtractive particles*; theſe latter are always parallelipipedons. I ſhall ſpeedily mention whence they derive their name. They are ſubſtituted for the integrant particles, to facilitate calculations, and it is worthy of obſervation that the parallelipipedon can always be obtained in all diſſections of cryſtals. Thus far, Sir, I have ſtated the firſt principles of mineralogical analyſis; I ſhall now proceed to the ſyntheſis.

(*To be continued.*)

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* The inclination of the interſecting planes being the ſame.

Deſcription

VIII.

Deſcription of a Filtering Machine invented by Profeſſor Parrot of Paris. In a Letter from a Correſpondent.

To Mr. NICHOLSON.

SIR,

Filtering machine by ſand in a reverſed tube.

I ENCLOSE you a drawing of a filtering machine invented by Profeſſor Parrot, of Paris, which for ſimplicity and utility ſeems ſuperior to any other I have met with. You will perceive that from the curvature of its form, it purifies the water both by deſcent and aſcent, and is, conſequently, a cloſer imitation of the operations of nature than thoſe in which the water penetrates in but one direction. Among the advantages which he aſcribes to it, he inſtances the "prolongation of the ſtratum of ſand, which does not conſiderably diminiſh the product of the filtre, but contributes remarkably to the purity of the fluid," and that "the difference of its water-level has an eſſential influence on the quantity of purified water obtained in a given time;" he therefore recommends an apparatus of eighteen inches long from A to D, two inches thick, and four broad, which, he ſays, will yield ſix (Paris) pints of pure water every hour: a machine of this ſize requires only a difference of two or three inches in the height of the water.

I am, Sir,
Your conſtant reader,

London, Auguſt 20, 1804. A. A.

Deſcription of the Machine.

The reſervoir G. (*Fig.* 1, *Plate* IV.) may be of any form or dimenſion which is convenient; the principal part of the machinery conſiſting of a ſquare veſſel bent in the form of an inverted ſyphon. The curve may be circular, elliptic, or in any other direction. This veſſel is to be filled with fine pure ſand to nearly the height of the dotted line $x\,y$, which denotes the aſcent of the water to D, whence it flows into the receiver. To the part marked A B, which muſt always be above this line, a woollen bag is attached, open at the top and reaching to the ſand: this collects the coarſeſt impurities, and prevents the ſand from becoming foul for a longer time. In large machines a water-tight trap-door may be made at F, for the purpoſe of removing the ſand when it is overcharged with impurities. The ſmall diameter of the machine from which the drawing was taken, was eight inches from B to E: the perpendicular height from C to A B was eight inches and three-fourths, and from C to D four inches and one-twelfth.

IX.

Medico-Chemical Reſearches on the Virtues and Principles of Cantharides. *By* H. BEAUPOIL.

(Concluded from Page 71 of Vol. VIII.)

Experiments and obſervations on cantharides.

HE alſo proved that the black precipitate eaſily became dry, brittle, and friable in the air; that it reddened the tincture of turnſole; that it combined very readily with potaſh, diſengaging ammonia; that, when diſtilled by an open fire, it ſwelled and yielded an acid liquor, a thick oil, and carbonate of ammonia; and that it left a dry, ſhining, friable coal in the retort.

Proceeding afterwards to the examination of the yellow matter remaining in ſolution in the alcohol, Cit. Beaupoil informs us, that, when it is concentrated by the evaporation of its ſolvent, it retains the ſame odour and the ſame taſte as the extract; that it is completely diſſolved in water, and reddens the tincture of turnſole; that it combines entirely with potaſh, without any diſengagement of ammonia; and that the reſult of this combination is an homogeneous and glutinous body, ſoluble in water and precipitable by a weak acid; finally, that, diſtilled by an open fire, it ſwells very little, yields an acid liquor, a black and fetid oil, and carbonate of ammonia; but that, in general, all theſe products are in ſmaller quantity than in thoſe obtained from the black precipitate.

Among theſe different reſults the author thought it neceſſary to direct his attention more particularly to the acid, which, as has been ſeen, manifeſts itſelf ſo readily in the infuſion of cantharides, or in the extract which they afford.

At firſt he was of opinion that this acid was analogous to that of vinegar; he alſo thought that its exiſtence might be attri-

buted

Experiments and obſervations on cantharides.

buted to the cuſtom prevalent with thoſe who collect the cantharides, of expoſing them to the vapour of this acid; but when he found, on ſubjecting ſome of theſe inſects which had been procured without the aſſiſtance of vinegar to experiment, that they were ſimilar to thoſe of commerce, he was obliged to renounce his firſt idea, and to endeavour to aſcertain the nature of the acid they offered him, by experiment. It appears that his endeavours have, in this inſtance, been unſucceſsful; for he finiſhes by inferring that his progreſs is not ſufficiently advanced to determine with certainty; and that, although the acid in queſtion has ſome analogy with the phoſphoric, he, nevertheleſs, does not think that it poſſeſſes all its properties, and, conſequently, is of opinion it ſhould be conſidered as a peculiar ſpecies, until new experiments have ſhewn that to which it in reality belongs.

The third product of cantharides, called by Thouvenel and by Cit. Beaupoil, green matter, does not ſeem to experience any change from the air, at leaſt in its phyſical properties. It is inſoluble in cold water; it liquefies in warm water, floating on its ſurface like an oil; alcohol and ether diſſolve it, and its ſolution in theſe two menſtrua is decompoſed by water. Oxigenated muriatic acid brought into contact with this matter, and renewed from time to time, at firſt ſeems not to have any action on it, but at length ſmall whitiſh, brilliant particles are detached from it, which fall to the bottom of the veſſel: in leſs than a week it loſes its ſmell and its colour, and becomes thick and glutinous; and notwithſtanding repeated waſhings, it conſtantly retains the odour of the oxigenated muriatic acid.

Diluted nitric acid, aſſiſted by heat, gives it a ruddy colour, a rancid, penetrating odour, and alſo a conſiderable conſiſtence.

Cauſtic ſoda combines with it without the aid of heat, and without a diſengagement of ammonia. The product of this union is decompoſed by the acids.

Expoſed to an elevated heat it fuſes, and forms a liquid of an oily appearance and ſlightly tranſparent, which, by cooling, quickly reſumes the ſolid ſtate. By a more powerful heat it is decompoſed, its colour changes, a yellowiſh oil, very analogous to that obtained from the diſtillation of wax, and an acid phlegm, paſs into the receivers, but not an atom of carbonate of ammonia.

Experiments & obſervations on cantharides.

With reſpect to the parenchyma forming the reſidue of the different macerations, infuſions, and decoctions in water, alcohol, and ether, the author, after having aſcertained that theſe fluids were incapable of extracting any thing more, treated it with cauſtic potaſh, which immediately cauſed a diſengagement of a very ſenſible ammoniacal odour. When this odour was diſſipated, the liquor was filtered, and inſtantly mixed with muriatic acid: the mixture became turbid, and gradually yielded a precipitate, which, dried and thrown on burning coals, exhaled an odour ſimilar to that of animal matters in combuſtion.

Diſtilled in a retort, this parenchyma yielded phlegm, a denſe empyreumatic oil, and a conſiderable quantity of carbonate of ammonia. The reſidue of the diſtillation was a ſpecies of coal, from which a white aſh was obtained by incineration in the open air, in which were found carbonate of lime, calcareous phoſphate, ſulphate and muriate of lime, and, finally, oxide of iron.

In recapitulating the quantities of each of the products obtained by means of the experiments which have been cited, the author aſſerts that one ounce of cantharides, well dried, contains nearly

Black matter	-	1 gros 2 grains;
Yellow matter	-	1 — 2
Green matter	-	1 — 8
Parenchyma	-	4 — 36
Acid	- -	An indeterminate quantity;
Calcareous phoſphate		12 grains;
Carbonate of lime	-	2
Sulphate and muriate of lime		4
Oxide of iron	-	2

To complete the work which Cit. Beaupoil had undertaken, it remained for him to determine the phyſiological properties of cantharides, as well as thoſe of the moſt eſſential of their immediate materials, and he appears to have executed this with ſucceſs in the fourth part of his diſſertation. Among other things, it reſults from the different experiments which he has made on this ſubject,

1*ſt*, That cantharides which have not undergone any preparation, almoſt always produce diſagreeable effects when taken

Experiments and obſervations on cantharides.

taken internally; but that theſe effects, with reſpect to their intenſity, are proportionate to the age, the ſtrength, and the conſtitution of the animals, and to the doſe which has been adminiſtered to them: That the œſophagus, the ſtomach, and the ſmaller inteſtines, are the parts which are principally affected: that thoſe animals which are not overcome by it, experience a deſire to vomit, very conſiderable pains and various affections, which ſeem plainly to indicate that the parts which have been touched by the cantharides, have a ſort of tendency to be diſorganized.

2d. That the aqueous extract of cantharides, in ſmaller doſes than the inſects themſelves, produces nearly the ſame effects as they do; and alſo, that its action on the urinary paſſages is very marked.

3d. That the black matter is much leſs active than the extract; that the animals to which it has been given, are only affected by gripings and vomitings, and very rarely are killed by it.

4th. That the green matter given internally does not appear to have deleterious qualities, ſince all the animals to which even ſtrong doſes had been adminiſtered, did not ſeem to be affected uneaſily by it.

5th. That the yellow matter does not ſeem to be more active than the green matter.

6th. That the extract, the yellow matter, and the black matter, applied ſeparately to the ſurface of the body, occaſioned veſication in nearly the ſame ſpace of time.

7th. That the green matter, applied externally, does not ſeem to act when alone; but that its action is ſpeedily manifeſted when it is divided by wax, and by that means receives the conſiſtence of a cerate.

I muſt not omit to mention, that Cit. Beaupoil was not ſatisfied with experiments made on animals, but had the courage to repeat them on himſelf. It was from having obtained the information he ſought in this manner, that he conſidered himſelf intitled to conclude, that the veſicating property reſides particularly in the extractive part and in the green part of the cantharides, but that the extractive part alone acts on the urinary and genital ſyſtem.

It will be obvious from the details I have given, that the author has carried the examination of cantharides farther than

Thouvenel,

Experiments and obſervations on cantharides.

Thouvenel. But although his work is greatly extended, it nevertheleſs is not yet complete, ſince much remains to be done, particularly with reſpect to the green matter; for it is difficult to conceive how it ſhould have no action on the animal economy when adminiſtered internally, ſince, applied externally, it produces a veſicating effect. This objection which I have made to the author, the importance of which he acknowledges, will doubtleſs be one of the motives which will determine him to renew his experiments to remove doubts, and to ſhew more clearly what is to be expected from the employment of the different parts compoſing an ingredient from which medicine has received ſuch great benefits.

X.

Experimental Determinations of the Latent Heat of Spermaceti, Bees' Wax, Tin, Biſmuth, Lead, Zinc, and Sulphur. By Mr. WILLIAM IRVINE. *Communicated by the Author.*

Bedford Street, Aug. 24, 1804.

Diſcovery of latent heat by Dr. Black.

IT will ſcarcely be denied that the diſcovery of the exiſtence of latent heat in all fluid and vaporous bodies, is one of the moſt curious and important hitherto developed in the progreſs of chemical philoſophy. The merit of firſt inveſtigating this ſubject is univerſally attributed to the celebrated Black. By a few ſimple and clear experiments he demonſtrated, that, before any portion of ice can become water, it muſt receive or abſorb as much heat as would have raiſed the temperature of an equal quantity of water by 140°. By other experiments, in ſome of which Dr. Black was aſſiſted by my father and Mr. Watt, it was proved in a manner equally ſatisfactory, that water cannot be converted into ſteam unleſs it admit a quantity of heat ſufficient to have heated the water 8 or 900°. Having proceeded ſo far by experiment, Dr. Black made a general inference, and extended his theory to all caſes of fuſion and vaporization whatever.

Dr. Irvine's examination of the latent heat of other bodies beſides water.

The only other philoſopher, as far as I know, who has attempted to determine the exact quantity of the latent heat in other bodies beſides water, was Dr. Irvine. Landriani made ſome

Spermaceti 145°, bees' wax 175°, tin 500°.

ſome experiments to prove that the fluidity of alum, ſulphur, and ſome metals, was accompanied with an abſorption of latent heat; but I believe he made no attempts to aſcertain the preciſe quantity. In Dr. Black's lectures we are informed, that Dr. Irvine found the latent heat of ſpermaceti to be 145°, of bees' wax to be 175°, and of tin to be 500°. From the very imperfect notes which I poſſeſs of the methods uſed to determine the two former, I believe that the 145° are meaſured by the capacity of fluid ſpermaceti, and the 175° by that of fluid wax: But of this ſubject I will take another opportunity to treat more amply. I am perfectly ignorant of every circumſtance regarding the latent heat of tin as determined by my father, having been unable to find any notes of his experiments for determining this point.

Experiments made by the author.

During the courſe of the preſent year I have turned my attention a little to this ſubject, I mean to the inveſtigation of the quantity of latent heat neceſſary for the fuſion of various bodies. I was perſuaded that an addition to our knowledge of latent heat would at leaſt increaſe the ſtore of facts, and might perhaps give riſe to ſome improvement or correction of theory. The veſſels which I employed in all my experiments were Florence flaſks, of which the neck was cut off. In theſe the water made uſe of was contained, and the veſſel was ſupported on a ſlight wooden ſtand, which preſented a very ſmall ſurface to abſtract heat from the materials. The orifice at the ſuperior part of the veſſel, was in general not more than ſufficient to permit the ready introduction of the fluid examined; probably from an inch and a quarter in diameter to a little more, ſo that a very ſmall ſurface of the water was expoſed to the air. The weight of the glaſs was in all caſes previouſly aſcertained. Theſe circumſtances being premiſed, I proceeded as follows:

The veſſels.

Determination of the latent heat of fluid biſmuth (or the number of degrees through which any quantity of biſmuth would, by the heat it gives out in congelation, raiſe an equal quantity of biſmuth, from beneath the freezing point of the metal).

The firſt ſubſtance which I ſubmitted to examination was biſmuth. The melting point of this metal was, by the thermometer which I uſed, 480°. This is ſo near the point found by my father, to wit 476°, that I ſhall conſider his determination as correct, as he combined and compared the different ways of computation and obſervation, and I know the thermometer which he uſed to have been made with conſiderable care. Into a glaſs veſſel which weighed 411 grains, I put 2236 grains of water, of the temperature of 62°. I then removed from the fire a quantity of fluid biſmuth. I waited till it was partially ſolid, at which time I inferred that both the ſolid and fluid metal were of the temperature of 476°. I next poured a portion of the fluid biſmuth into the water. In one minute the thermometer ſtood at 86°; in two minutes at 85¾: The true temperature at the moment of mixture was therefore 86¼, ſuppoſing equal temperatures to be loſt in equal intervals. There was alſo a quantity of ſteam formed. I weighed the veſſel with its contents, and found that it had gained 1589 grains. This therefore ſhould be the quantity of biſmuth poured in, if there had been no loſs. But on drying and weighing the metal, it appeared to amount only to 1555 grains, and 34 grains conſequently were loſt. The biſmuth was cooled 389¾°, the water was heated 24¼°. Then 1555 grains of water would have gained 34°. Theſe 34°, meaſured by the ſpecific heat of biſmuth, as ſtated in Thomſon's Syſtem of Chemiſtry at .042, are equal to 810°.9: But the biſmuth, after becoming ſolid, loſt 389°.75, which being ſubtracted from 810.9, there remains 421°.15, which cannot be accounted for by the cooling of the ſolid biſmuth, and muſt therefore be the whole or a part of the latent heat of the fuſed metal.

Biſmuth, on the point of congealing, was poured into water, the quantities, temperatures, &c. being known; and the common temperature was taken.

Inference of the latent heat.

But the latent heat muſt be greater than this, for 411 grains of glaſs were alſo heated 24°.25. If the capacity of this glaſs be taken at .174, as Kirwan found flint glaſs to have, and I have found green bottle glaſs to have a capacity of .173 by ſeveral experiments, whence it is probable that the glaſs of Florence flaſks has its capacity not very wide of theſe numbers; if then .174 be taken as the capacity of this glaſs, theſe 24°.25, which the biſmuth communicated to the 411 grains of glaſs in the veſſel, are, when meaſured by the capacity of biſmuth, equal to 96°.4: And 1555 grains of glaſs would have gained 25°.4, which muſt be added to 421°.15 already found, and makes 446°.55 for the latent heat of biſmuth.

Correction, for the veſſel;

But this is obviouſly ſtill too little; for, as has been already mentioned, there was a good deal of ſteam formed. The amount of the heat thus loſt is extremely difficult to aſſign. I ſhall, however, make an attempt to gueſs at, rather than determine it. All the 34 grains muſt not be reckoned to have been loſt by evaporation. In ſpite of all my efforts, I could perceive that ſome, though certainly a ſmall quantity of biſmuth went off with the water, in form of a number of ſmall particles floating in the liquid: and perhaps alſo a ſmall portion

and for ſteam produced.

tion of water might be waſted during the proceſs. If we allow a half of all the loſs to be accounted for in theſe ways, there remain ſixteen grains of water which have been converted into ſteam. The latent heat of ſteam was computed, by Mr. Watt, to be equal to 940°: But this ſteam cannot be allowed ſo much heat as this; and though it may be difficult to point the quantity to be fairly granted, yet I ſhall expect to be within bounds when I eſtimate it at one half of 940°, or 470°. In this caſe we ſay, 16 grains of water have been heated 470°; ſixteen grains of biſmuth would be heated by the ſame quantity of caloric, 11190°.4; and 1555 grains + 16 grains ſuppoſed to have gone off with the water, = 1571 grains of biſmuth, would be heated 113°.9. This quantity of heat, therefore, ought to be added to the 446°.55 already found, and would amount, in all, to 560.45 latent heat of biſmuth.

Latent heat of biſmuth deduced 560¼ degrees.

Other experiments with biſmuth.

I am ſenſible that there are ſeveral gratuitous ſuppoſitions in this laſt part of the reaſoning, and I do not therefore lay much ſtreſs on it. I endeavoured to confirm or refute the truth of the inductions, by making an experiment exactly on the ſame principles, but where, by dexterity, I might prevent the formation of ſteam wholly or in part. In one inſtance I ſucceeded tolerably well, and then the latent heat, with every correction, amounted to nearly 600°.

I repeated theſe experiments for determining the latent heat of biſmuth many times, and the reſult is expreſſed in the following

TABLE:

Tabulated.

No. of Experiments.	Latent Heat by firſt Computation.	Correction for the Heat received by Glaſs.	Whole Latent Heat.
1	457	23	480
2	411	29	440
3	412	28	440
4	465	33	498
5	480	29	509
6	438	27	465
7	465	30	495
Mean	446.8		475

On the whole, therefore, it appears that we ſhall not exceed the truth when we eſtimate the latent heat of biſmuth at 550°.

Mean reſult 550° for the latent heat of biſmuth.

I made, in like manner, two experiments to aſcertain the latent heat of tin, and of theſe the reſults were—

Experiments on tin. Latent heat 507°.

Expt.		
1	520	Mean, 507°.
2	495	

This agrees remarkably well with the determination of this point, ſaid to have been made by my father, in Black's Lectures.

Zinc is computed, by Bergman, to melt at 700° of Fahrenheit's thermometer. Taking that for granted, I made three experiments on the latent heat of zinc, in a ſimilar way with that already related with biſmuth. The reſults were as follow:

Latent heat of zinc 493°.

Experiment.	Latent Heat by firſt Computation.	Corrected.	Whole L. H.
1	490	28	518
2	476	22	498
3	443	32	475
Mean	469		493

In theſe experiments no allowance is made for loſs by ſteam, which, however, by the dexterity acquired by practice, I was enabled to render very ſmall.

Latent heat of lead 146°.

Lead I found to melt at a point above 584°. Owing to the ſhortneſs of the thermometric ſcale, I could obſerve no higher. I ſuppoſe therefore that 594°, as found by Dr. Irvine, cannot be materially diſtant from the truth. Aſſuming it as true, I proceeded to make ſeveral experiments on the latent heat of lead. In doing this I was led immediately to notice, that melted lead does not by any means produce ſo much ſteam as other metals do when poured into water, even under the moſt careleſs management; and of this the reaſon will appear from the following table:

Experiments on the Latent Heat of Lead.

No.	Latent Heat.	Correcţed.	Whole Latent Heat.
1	127.8	10°.3	138°.1
2	142.8	11°.	153°.8
3	149.9		
4	161.8		
5	131.5		
Mean	142.7	Mean	145°.9

In theſe experiments I have, by accident, loſt the notes of the determination of the weight of the veſſel employed in all but the two firſt inſtances: notwithſtanding which it may be fairly inferred, that the mean latent heat of lead is about 150°, a quantity certainly unexpectedly ſmall, and which, in many more experiments than theſe related, I was at pains to examine, without being able to diſcover any material inaccuracy. In the ſecond experiment, where a little ſteam was formed, I aſcertained the loſs to be four grains. If theſe four grains be ſuppoſed, as in the caſe of biſmuth, to contain 470° of heat, the computed addition to the latent heat of lead will be 20°.3, making in all a little more than 162°. This is certainly a very peculiar and unexpected quality of this metal.

Experiments on ſulphur. The only other ſubſtance which I have ſubmitted to examination for the purpoſe of aſcertaining its caloric of fluidity, is ſulphur. The melting point of ſulphur is commonly ſtated, in elementary works, to be at 212°: But that this is not accurate any one may convince himſelf, by immerſing a quantity of ſulphur in boiling water, where it remains altogether unaltered. By every trial which I have been able to make, I am convinced that the fuſion of ſulphur takes place about the temperature of 226°. I ſay about, becauſe the communication of heat among the particles of ſulphur is very ſlow, and the thermometer is often encruſted with ſolid ſulphur, which, ſome how or other, certainly cools below the liquid in which it is immerſed. In experiments for aſcertaining this point, the thermometer ought to be kept in conſtant motion. The reſults of my experiments for finding the latent heat of ſulphur, are ſtated in the following table.

Experiments to aſcertain the Latent Heat of Sulphur.

Latent heat of ſulphur 143½°.

No.	Latent Heat.	Corrected.	Whole Latent Heat.
1	144°.56	8°.	152°.56
2	131°	7°.	138°.
3	140°	4°.5	140°.5
4	136°		
Mean	137°.89	Mean	143°.68

In theſe experiments I have ſuppoſed, from experiments of my own, the capacity of ſulphur to be .189, which does not materially differ from Mr. Kirwan's determination. In the other caſes I have truſted chiefly to the numbers given in Thomſon's Chemiſtry, though theſe are not always wholly unexceptionable. In every inſtance I have ſuppoſed the ſemi-liquid to have the temperature of the melting point, which I believe is generally true; but ſome practice is required to ſeize the moment before the frozen particles float in the fluid ſubſtance. In experiments on ſulphur eſpecially, inattention to this circumſtance cauſes very great inaccuracy, and was the cauſe of conſiderable embarraſſment to me before I obſerved my error.

A comparative table follows of the caloric of fluidity of all ſubſtances hitherto examined:

Table of latent heats.

Subſtance.	Melting Point.	Latent Heat.	Ditto in Degrees meaſured by Capacity of Water.
Ice	32°	155°.555	140°
Spermaceti	113°	145°	
Bees' Wax	142°	175°	
Tin	442°	500°	33°
Biſmuth	476°	550°	23°.65
Lead	594°	162°	5°.604
Zinc	700°	493°	48°.3
Sulphur	226°	143°.68	27°.145

General remarks.

In all theſe inſtances the latent heat is expreſſed in degrees meaſured by the capacity of the relative ſolid, excepting in the caſes of ſpermaceti and bees' wax, which are in degrees meaſured by the capacity of the fluid. I endeavoured to rectify tiſy this ſo as to make the compariſon more fair, by determining the ſpecific heat of ſolid wax and ſpermaceti; but I have not been able to ſatisfy myſelf with either of theſe points, owing to the ſoftneſs and conſequent abſorption of latent heat, which a very low degree of heat induces in both theſe bodies. The numbers expreſſing their latent heats are therefore too low. On inſpection of the above table, there does not appear any ratio by which the quantity of the caloric of fluidity ſeems to be guided: it certainly does not increaſe with the difficulty of fuſion, but moſt probably has ſome connection with the relative capacity of each body in its ſolid and fluid ſtate. The determination, however, of the capacity of any of the metals in a fluid form, excepting mercury, muſt be regarded as an extremely difficult taſk.

XI.

Strictures on Mr. DALTON'S *Doctrine of Mixed Gaſes, and an Anſwer to Mr.* HENRY'S *Defence of the ſame. In a letter from Mr.* JOHN GOUGH.

To Mr. NICHOLSON.

SIR,

Cauſes of theſe ſtrictures.

I HAVE ventured to defend the chemical union of water and air, as well as the homogenity of the atmoſpherical gas. My thoughts on theſe ſubjects are briefly ſtated in your Journal *; and the farther proſecution of the enquiry compels me to make an open attack on my friend Mr. Dalton, and his new convert, Mr. Henry. The diſpute ſhall be fairly conducted on my part; that is, it ſhall conſiſt of arguments which I am ready to abandon as ſoon as they are refuted; this promiſe is due to friendſhip, as well as the obligations of truth.

Dalton's a hypotheſis not a theory.

The firſt thing to be aſcertained is the proper appellation of Mr. Dalton's opinions. His doctrine of mixed gaſes is offered to the public as a mechanical theory, founded on chemical facts: a little attention, however, to Mr. Dalton's eſſays, will deprive it of all claim to the title of a theory. This is evident from the nature of the mechanical philoſophy; every branch of which admits of a mathematical demonſtration, derived from Newton's definitions and laws of motion: but Mr. Dalton has not attempted to give ſtability to his new ideas, by the aid of the mathematics; on which account the only appellation due to his doctrine, is that of a hypotheſis.

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* Vol. VIII. Page 243.

The hypotheſis not mechanical.

No philoſopher ought to diſregard the means which are able to confirm his opinions, becauſe theſe means may be found, upon trial, to ſubvert them. This I believe to be the caſe with my friend's hypotheſis; for I have endeavoured to ſhew the fallacy of it by mathematical arguments. The reſult of this attempt will in all probability appear in the next volume of the Mancheſter Memoirs; when the merits of the eſſay will be determined by geometricians, who are the proper judges of ſuch productions. This want of geometrical demonſtration eſcapes the notice of the chemiſt, becauſe my friend has ſeemingly ſupplied the deficiency by a number of probabilities of an experimental nature; but it is almoſt ſuperfluous to remind either him or your readers, that a myriad of ſuch proofs cannot uphold a doctrine which is repugnant to the mechanical philoſophy.

Concluſions from theſe probabilities.

The two leading maxims which are derived from theſe probabilities, and form the baſis of the hypotheſis, are thus briefly expreſſed by Mr. Henry: "mixed gaſes neither attract nor repel each other, and every gas is as a vacuum to every other gas[*]". Mr. Dalton, reaſoning from theſe premiſes, ſurrounds our globe with an independent atmoſphere of vapour, the preſſure of which preſerves all the water on the earth's ſurface in a liquid ſtate, and prevents the ocean itſelf from eſcaping through the air, by virtue of its own elaſtic force; at leaſt the laſt is a fair inference from his own concluſions.

Dalton's reaſoning incomplete,

Although I have demonſtrated the exiſtence of an atmoſphere of vapour to be a mechanical impoſſibility, in the eſſay mentioned above, the reality of the thing ſhall be ſuppoſed at preſent, for the purpoſe of detecting the fallacy of the doctrine in the fundamental maxims of it. The truth is, Mr. Dalton has diſcontinued his train of reaſoning too ſoon; for, had his arguments been purſued to their proper limits, they would have diſcovered the incompatibility of the hypotheſis and natural appearances. This omiſſion is eaſily ſupplied, and will be as eaſily comprehended, by a perſon who underſtands the laws of hydroſtatics.

[*] Phil. Journ. Vol. VIII. Page 298.

A ſecond particle may follow a former.

Air preſſes upon water.

Air a perforated piſton, by the hypotheſis.

Air not a vacuum to vapour.

If a particle of vapour can paſs freely through the air, a ſecond can alſo ſucceed it at any given diſtance; becauſe the latter may undoubtedly purſue the track which the former has already traverſed: conſequently, a ſeries of ſuch particles, poſſeſſing the denſity of water, might be raiſed into each perpendicular pore of the atmoſphere, by the application of a proper force to the ſurface of a collection of water ſupporting ſuch an atmoſphere. Now the exiſtence of ſuch a force is certain; becauſe when air is injected into either leg of an inverted ſyphon containing water, it conſtantly diſturbs the equilibrium of this fluid; that is, the gas preſſes upon the liquid, notwithſtanding the ſuppoſition, that the former is a vacuum to the particles conſtituting the latter. If now we combine the poſtulates of the hypotheſis with the preceding fact, a column of air, which occupies the upper part of a veſſel containing water in its bottom, becomes a heavy piſton, having its ſubſtance perforated in every direction by pores of eaſy tranſmiſſion, which are, at the ſame time, ſeparated by partitions impervious to water. After contemplating this imaginary ſtructure of the atmoſpherical gas, let the reader inveſtigate the conſequences of it, and compare them with natural events. The impenetrable parts of the gaſeous piſton would compel the water to aſcend along its perpendicular pores; in which it would be kept duly condenſed by the preſſure of the incumbent vapour. The ſpecific gravity of this column, compounded of air and water, would exceed that of the external air; conſequently the upper extremity of it would conſtantly glide over the edges of the veſſel into the atmoſphere. Thus there would happen a double loſs of water, namely, by evaporation and percolation: but nothing of the kind is obſervable in nature; conſequently the air is impenetrable to the conſtituent particles of water not heated to the boiling point.

Impenetrability no obſtacle to chemical union.

One gas not a vacuum to another.

The circumſtance of water not being able to penetrate air at low temperatures, is no obſtacle to the chemical union of the two ſubſtances; for many aqueous ſolutions of ſalts occupy leſs ſpace, when completed, than the materials formerly did of which they are compoſed. This fact has been eſtabliſhed by the preſent Biſhop of Llandaff; and it proves that bodies, which are mutually impenetrable, may be ſuſceptible of the bond of chemical affinity.

After all that has been ſaid about the conſtitution of gaſes, the notion is incorrect, ſtrictly ſpeaking, which ſuppoſes each gas to be as a vacuum to every other. The foregoing obſervation may alſo be extended to mixtures of carbonic acid gas with water; and the following inſtances may be adduced in confirmation of the propoſition: When a particular gas is developed in a veſſel containing common air, the firſt portion that comes over has in it leſs of the ſpecific gas than the ſecond; nor is the ſecond equal to the third in purity. This circumſtance ſhews, that the new-formed gas does not find a vacuum in the air of the veſſel; on the contrary, the two fluids produce a mechanical mixture, which is gradually expelled, until no part of the air remains in the tube or bottle. In like manner, if a bottle of highly ærated water be opened ſuddenly, the rapid expanſion of the liberated gas ejects a great part of the contents, thereby proving that water oppoſes an obſtacle to the dilatation of this gas. In reality, Mr. Dalton allows one gas to be an impediment to the motions of another; but at the ſame time he maintains, that two ſuch fluids finally overcome their mutual obſtructions, and occupy the ſame ſpace in a ſtate of perfect independence. This is a propoſition which may be juſtly ſuſpected of being a ſoleciſm in pneumatics, until the author of it has proved the contrary by a rigorous demonſtration.

Proved by experiment.

Amongſt the many probabilities which have been offered in ſupport of the hypotheſis, perhaps none are more ingenious than the remarks of Mr. Henry; and the part which I have taken in the preſent letter, obliges me to place them in a new light. This attempt muſt, however, be preceded by a theory, which will explain the relations of thoſe gaſes that neither attract nor repel each other: ſuch an explanation became a neceſſary part of pneumatics, from the time when Dr. Prieſtley made his experiments upon mixtures of this deſcription. The following, then, is a ſketch of a theory, having for its foundations the mutual repulſion of homogeneous particles, and the reciprocal reſiſtance which gaſes have been ſhewn to poſſeſs.

A new theory of mixed gaſes neceſſary.

When two ſuch gaſes come into contact, parcels of each will be detached by every ſlight force, and enveloped in the ſubſtance of the other. In this manner, the two fluids will be broken to pieces, and blended in one maſs forming a mechanical

Sketch of the theory.

nical mixture; the component parts of which cannot be ſeparated without the intervention of chemical agents, becauſe the disjoined fragments of each gas will be prevented from reuniting by their mutual repulſion. There is one exception, however, to the general rule; for when a liquid is found in a mixture, the component parts will follow the law of their ſpecific gravities; becauſe an inelaſtic fluid is not actuated by an intrinſic repulſion, in all other caſes the gaſeous fragments will continue to be farther and farther ſubdivided, and will conſtantly aſſume a new arrangement from the ſlighteſt agitations. The conſtituent parts of ſuch a mixture exert their force in perfect union; and this circumſtance diſtinguiſhes it from Mr. Dalton's compound, the conſtituent gaſes of which preſs ſeparately upon all ſurfaces.

Exception to the general rule.

The theory applied to Mr. Henry's experiments.

The neceſſary premiſes being now ſettled, it is time to try the powers of the theory upon Mr. Henry's experiments. If, then, ten meaſures of water, containing an equal bulk of carbonic acid gas, be preſſed by a column of the ſame gas, equivalent in force to 30 inches of mercury, the ſtate of the aqueous compound will remain invariable; becauſe the ſpring of the gas in the water appears, by Mr. Henry's experiments, to be equal to the ſpring of the incumbent gas: therefore, ſhould a gaſeous particle happen to eſcape from the compound, an equal particle, from the upper part of the veſſel, will replace it immediately. But if the incumbent carbonic acid be made to give place to a maſs of common air of the ſame elaſtic force, the ſurface of the aqueous compound will undergo no change of preſſure; but the gaſeous part of it, meeting with no repulſion from the column of air, will begin to form a mechanical mixture with it, according to the theory. The parcels of the carbonic acid will alſo remain diſtinct, according to the ſame, after their eſcape from the water; and as a number of them will be arranged on the ſurface dividing the two mediums, they will form ſo many obſtacles to the diſcharge of their kindred gas; the egreſs of which will be confined to the intermediate compartments of eaſier tranſmiſſion conſiſting of common air. The diviſion of the ſurface between the two mediums, into compartments of eaſy tranſmiſſion and impenetrable points, ultimately produces a ſort of equilibrium, which aſſigns their reſpective portions of the elaſtic acid, to the gaſeous compounds. The origin of this equilibrium is eaſily collected from the preceding theory and the laws of pneumatics. For the globules of carbonic acid, contained in the gaſeous compound, invariably exert the force of 30 inches of mercury; they therefore prove too powerful for the rarefied gas of the water, which they compreſs, and inſinuate part of their own ſubſtances into the fluid. Whilſt this operation is going on at innumerable points in the ſurface, portions of the ſame gas are conſtantly forced through the compartments of eaſy tranſmiſſion, by the ſlighteſt agitations. Now the quantity of gas which is diſcharged into the air of the jar, is greateſt at firſt, and continually diminiſhes; on the contrary, the quantity that is forced into the water through the former paſſages, is leaſt at firſt, and increaſes afterwards; conſequently the two quantities ultimately become equal, and eſtabliſh the equilibrium in queſtion, by the contrariety of their effects.

Concluding remarks.

The preceding is a general propoſition, which explains a variety of appearances, ſuch as Mr. Henry has deſcribed, by by the well known principles of mechanics. Should the conſideration of it be attended with conviction on his part, he perhaps will give a new turn to his experimental enquiries, and endeavour to diſcover the law of affinity, which connects water with the different gaſes. It is almoſt certain, that this law is not the ſame in all caſes, as will be ſeen by comparing the experiments of M. la Sauſſure on the ſolution of water in common air, with Mr. Kirwan's obſervations on the ſolvent powers of hidrogen; which may be found at page 14 of the firſt edition of his Eſſay on Phlogiſton.

JOHN GOUGH.

Middleſhaw, Aug. 23, 1804.

P. S. The following error requires correction: In Vol. VIII. page 244, line 21, before the words, *as oft as water*, read *therefore*, and ſtrike out the ſame conjunction in line 22.

An

XII.

An Enquiry concerning the Nature of Heat and the Modes of its Communication. By BENJAMIN COUNT *of Rumford, V. P. R. S. &c. Abridged from the Philoſophical Tranſactions for the Year* 1804.

Experiments on heat.

AFTER remarking that all diſcoveries on an agent of ſuch extended operation as heat cannot fail to be eminently uſeful, the author proceeds to deſcribe the apparatus uſed in the experiments now to be deſcribed: They were the following.

Inſtruments. Thermometers.

1. Mercurial thermometers carefully conſtructed, having cylindrical bulbs, four inches long and four tenths of an inch in diameter, and their tubes from 15 to 16 inches long; the air being excluded, and the graduations according to Fahrenheit, exhibiting eight parts of degrees by means of a nonius.

Inſtruments. Cylinders to contain hot water, and ſuffered to cool with different kinds of clothing.

2. Four cylindrical veſſels of thin ſheet braſs, for aſcertaining the warmth of clothing. *Fig.* 1. *Plate* I. The veſſel is cloſed at both ends; but has a neck at the top, into which hot water is occaſionally poured, and in which one of the thermometers is fitted and placed during the time of an experiment, ſo that its long bulb ſhall occupy the axis of the veſſel, and will ſhew its mean temperature. Another cylindrical neck proceeds from the lower ſurface, and is fitted upon the adjuſtable part of the wooden ſtem beneath. The veſſels are four inches long and four inches diameter, and the necks are about eight tenths of an inch in diameter, the upper one being four inches long and the lower three inches. When the veſſel is clothed and charged with hot water, its rate of cooling by expoſure to the quiet air of a large room, will ſhew the relative warmth of each particular kind of clothing.

In ſome of the experiments the ends of the inſtrument were permanently covered by the application of a thin wooden box to each, the box being varniſhed and covered with fine writing paper, and filled with fine eider down, and a cap of fur was pulled over the box, and the projecting neck. The clothing of theſe caſes was applied for experiments to the cylindrical ſurface.

Two of the inſtruments (No. 1 and 2.) were thus covered up at the ends, and the other two (No. 3 and 4) were left in the ſtate repreſented *Fig.* 1. without the permanent coverings.

In

Method of op ating with the cylinders to ſh the effect of clothing.

In each experiment two ſimilar inſtruments (ſuppoſe No. 1 and No. 2, or No. 3 and No. 4) were uſed, one *naked* and the other *covered*; ſo that in each experiment the naked inſtrument ſerved as the ſtandard of compariſon with the other.

The experiments were made and regiſtered in the following manner: the two inſtruments uſed in the experiment, placed over their wooden ſtands, being ſet down on the floor, were filled to within about 1½ inch of the tops of their cylindrical necks with boiling hot water; and a thermometer being put into each of them, they were placed at the diſtance of three feet from each other, on a large table in a corner of a large quiet room, 24 feet long, 19 feet wide and 13 feet high, where they were ſuffered to cool undiſturbed. Near them, on the ſame table, and at the ſame height above the table, there was placed another thermometer, ſuſpended in the air to the arm of a ſtand, to aſcertain the temperature of the air.

Every caution was uſed to prevent diſturbance by currents or agitation of the air, whether by partial heat or the intruſion of any perſon during the progreſs of any experiment.

Scale through which the refi geration was meaſured.

By the reſults of a great number of experiments, it was found that the ſame inſtrument cooled through any given (ſmall) number of degrees, for inſtance 10°, in very nearly the ſame time, whatever was the temperature of the air of the room; provided always that the point from which theſe 10 degrees commenced, was at ſome conſtant number of degrees above the temperature of the air at the time being. The interval choſen by the Count lay between the 50th and the 40th degrees above the temperature of the air in which the inſtrument was expoſed to cool; when for inſtance, the air was at 58°, the interval commenced at 108, and ended at 98°.—When the air was at 64½° it commenced at 114½° and ended at 104½°.

The warmth of any covering, or its power to confine heat, was eſtimated by the time employed in cooling through that interval.

Method of ſu plying by inte polation the e treme inſtants time elapſed, when omitted be obſerved.

As it ſometimes happened, though very ſeldom, in the courſe of an experiment (which commonly laſted ſeveral hours) that the moment of the paſſage of the mercury through one or both of theſe extreme points was not obſerved, it was of importance to determine the ſame by interpolation from the other points obſerved. To do this, the author availed himſelf of the following law of cooling of hot bodies in a fluid, which he found by experiment to be applicable without ſenſible error in the preſent caſe: It is that, if the equal portions of a right line repreſent ſucceſſive intervals of time, and perpendiculars be erected upon the ſame, to denote by their lengths the degrees of the exceſs of temperature of the hot body beyond that of the cold medium, at points denoting the correſponding inſtants of time, the line joining the extremities of the perpendiculars will be the logarithmic curve. Whence if two temperatures and the elapſed time be obſerved, it will be eaſy by the help of a table of logarithms to determine the time at which any intermediate temperature took place. This is exemplied by the Count, who then proceeds to relate his experiments.

Exp. 1. A clothing of Iriſh linen cauſes the heat to eſcape much quicker than from a bright metallic ſurface.

Exp. 1. One of the veſſels, No. 1. having its ends clothed as before deſcribed, and its poliſhed ſides naked, was filled with hot water. Another veſſel, No. 2. alike in all reſpects, but having its ſides cloſely clothed with Iriſh linen, ſuch as is ſold in London at 4*s.* per yard, was filled in like manner, and both were ſimilarly expoſed to cool.

The naked inſtrument employed 55 minutes in cooling, from 94° to 84° Fahrenheit (the air of the room being at 45°.)—But the clothed inſtrument was cooled through the ſame interval in 36½ minutes, conſequently this clothing expedited the emiſſion of heat inſtead of confining it.

When both inſtruments were cooled to 42° they were removed into a warmer room at 62°, and the clothed inſtrument was alſo found to acquire heat conſiderably faſter than the other.

Whether the linen accelerated the cooling by aſſiſting the ſucceſſion of freſh particles of air, or by promoting the eſcape of heat by radiation, were points to be determined.

Exp. 2. The ſame effect from a coating of glue.

Exp. 2. To decide this, the linen of No. 2. was removed, and the ſides were thinly coated with glue. In theſe circumſtances, while the ſtandard or naked inſtrument cooled through the interval in 55 minutes, the coated inſtrument employed only 43¼ minutes.

In reaſoning upon this experiment, the author concluded, that if the glue operated only by preventing the air from attaching and fixing itſelf to the poliſhed metallic ſurface, and conſequently in that manner facilitated its circulation and the cooling; it would be of no conſequence, whether the ſurface was covered by one or more coatings of glue: But, on the contrary, if the radiations of heat were facilitated and increaſed, it might be expected that a greater effect would be produced by two coatings than by one.

Exp. 3. More coatings of glue increaſed the effect.

Exp. 3. The experiment was therefore tried, and it was found that the inſtrument uſed employed only $37\frac{5}{8}$ minutes in paſſing through the interval.

Exp. 4, 5, 6, 7. Alſo ſpirit varniſh,

Exp. 4, 5, 6, 7. When the experiment was repeated with clear colourleſs ſpirit varniſh, the ſame effect was produced, and it was augmented as far as by four coatings. But on proceeding as far as eight coatings, the limit of the greateſt effect was found to have been paſſed.

Exp. 8, 9, 10. —and black ſize paint, —and white,

Exp. 8, 9, 10. Black paint (lamp black and ſize) upon the varniſh increaſed the cooling effect a little. When the inſtrument was cleaned and then painted, its rapidity of cooling was nearly the ſame = 35 minutes. And with white paint the difference was not conſiderable, as the effect was produced in 36 minutes. As the paint was laid on in ſeveral ſucceſſive coatings, the Count remarks that little dependence is to be placed on theſe reſults as indicating a difference from colour.

Exp. 11. And of ſmoke from a candle.

Exp. 11. The clean inſtrument being ſmoked black over a wax candle, was found to have cooled through the interval in $36\frac{1}{8}$ minutes, while the ſtandard employed $55\frac{7}{8}$ minuteſ. The lamp black, when wiped off and weighed, amounted to leſs than $\frac{1}{15}$ of a grain, though it had completely covered 50 ſquare inches.

Determination of the eſcape of heat at the flat ends of the inſtruments.

With a view to a more accurate determination of the velocity of cooling, it was neceſſary to aſcertain what heat eſcaped through the permanent clothing at the ends. This was done by obſerving the times of cooling with and without the clothing, and comparing the effects with the reſpective ſurfaces of expoſure. For the whole ſurface of the inſtrument 85.195 ſquare inches, is to the ſurface of its vertical ſides; ſo is the whole quantity of heat paſſed off = 10000 to the quantity that paſſed through the ſame ſides = 5885. And ſince by obſervation the naked inſtrument required $45\frac{1}{2}$ minutes to cool through the ſame interval as was paſſed through in $55\frac{1}{2}$ minutes when the ends were covered, the author concludes that $45\frac{1}{2} : 55\frac{1}{2} :: 5885 : 7015 =$ what would have paſſed in the later interval through the upright ſurface, and conſequently that the remainder of the heat = 2985 parts muſt have paſſed through the covered parts of the inſtrument.

By applying theſe reſults to the numbers in *Exp.* 11. where the cooling was effected in $36\frac{1}{8}$ minutes, the Count ſays as $55\frac{1}{2}$ minutes give 2985 heat paſſed through the covered ends, ſo will $36\frac{1}{8}$ minutes give 1942 parts. And this taken from 10000, the whole heat loſt, will leave 8058 for the heat that really paſſed through the upright ſides. But it was found that 7015 paſs through the naked ſides in $55\frac{1}{2}$ minutes: Whence $7015 : 55\frac{1}{2} :: 8058 : 63\frac{3}{4}$. And conſequently, the corrected times are $36\frac{1}{8}$ and $65\frac{1}{4}$, which expreſs the velocities of the paſſage of heat through the ſurface of the naked metal, and that which was blackened with ſmoke, viz. as 5654 to 10000 nearly.

Whence the reſults are corrected.

In the ſame manner the velocities of the paſſage of heat in the experiment No. 6. are ſhewn to have been as 4566 to 10000.

A new courſe of experiments.

It has been remarked that, in theſe curious inſtances of the effects of modification of ſurface or clothing upon the tranſitions of heat, the effect may have been favoured by communication to the air, or by facilitating the proceſs of radiation. The author's reaſoning upon *Exp.* 2. appeared not ſo deciſive as to need no ſupport from experiments of a different claſs. He therefore conſtructed an inſtrument for meaſuring the effects of radiation, which is ſeen in *Fig.* 2, *Plate* I.

Inſtruments for improving radiant heat.

Like the hygrometer of Mr. Leſlie,* (as the Count obſerves) it conſiſts of two glaſs balls at the ends of a tube C and E. The tube is of ſuch a diameter that one inch in length would contain 15 grains of mercury; the balls are 1.625 inches in diameter; the upright ends of the tube C and E are each 10 inches long; the horizontal part D is 17 inches; and the board A B, to which it is attached, is 27 inches long, 9 inches wide, and one inch thick. The pillar F ſupports a circular vertical ſcreen made of paſteboard, covered with gilt paper on both ſides, the uſe of which is to protect one of the balls from rays intended only to act upon the other. The balls contain only air, and a ſmall drop of coloured ſpirit of wine is introduced by means of a ſhort tube projecting from one of the elbows; which ſhort tube is then hermetically ſealed. By a little management the bubble of ſpirit is brought to reſt in the middle of the horizontal tube; and when the temperature of the air in either of the balls is made to exceed that of the other, the increaſed elaſticity cauſes the bubble to move towards the colder ball.

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* Philoſophical Journal, quarto ſeries, III. 461.

By a ſimple contrivance of ſliding boards, the hot bodies were moved by rack-work and a winch to any diſtance from the thermoſcope without the attention of the obſerver being taken off from the bubble in the tube, and the diſtances were alſo ſhewn by a graduated ſcale and nonius. *Fig.* 3. *Plate* I. ſhews one of theſe bodies. It is a metallic cylinder having its baſe, which is to be preſented to the thermoſcope, vertical, and its neck obliquely placed for the purpoſe of introducing the hot Water, and alſo a thermometer for ſhewing its temperature at any required time.

The experiments and obſervations which conſtitute the remainder of this memoir will be given in our next.

XIII.

Experiments and Calculations relative to phyſical Optics. By THOMAS YOUNG, *M. D. F. R. S. From the Philoſophical Tranſactions for* 1804.

I. EXPERIMENTAL DEMONSTRATION OF THE GENERAL LAW OF THE INTERFERENCE OF LIGHT.

IN making ſome experiments on the fringes of colours accompanying ſhadows, I have found ſo ſimple and ſo demonſtrative a proof of the general law of the interference of two portions of light, which I have already endeavoured to eſtabliſh, that I think it right to lay before the Royal Society, a ſhort ſtatement of the facts which appear to me ſo deciſive. The propoſition on which I mean to inſiſt at preſent, is ſimply this, that fringes of colours are produced by the interference of two portions of light; and I think it will not be denied by the moſt prejudiced, that the aſſertion is proved by the experiments I am about to relate, which may be repeated with great eaſe, whenever the ſun ſhines, and without any other apparatus than is at hand to every one.

General laws of the interference of light proved in the production of fringes of colours.

Exper. 1. I made a ſmall hole in a window-ſhutter, and covered it with a piece of thick paper, which I perforated with a fine

Exp. 1. Fringes bordering the ſhadow of a ſlip of card are produced by the interference of light from both edges. For interception on one ſide deſtroys the effect.

a fine needle. For greater convenience of obſervation, I placed a ſmall looking glaſs without the window-ſhutter, in ſuch a poſition as to reflect the ſun's light, in a direction nearly horizontal, upon the oppoſite wall, and to cauſe the cone of diverging light to paſs over a table, on which were ſeveral little ſcreens of card-paper. I brought into the ſun-beam a ſlip of card, about one-thirtieth of an inch in breadth, and obſerved its ſhadow, either on the wall, or on other cards held at different diſtances. Beſides the fringes of colours on each ſide of the ſhadow, the ſhadow itſelf was divided by ſimilar parallel fringes, of ſmaller dimenſions, differing in number, according to the diſtance at which the ſhadow was obſerved, but leaving the middle of the ſhadow always white. Now theſe fringes were the joint effects of the portions of light paſſing on each ſide of the ſlip of card, and inflected, or rather diffracted, into the ſhadow. For, a little ſcreen being placed a few inches from the card, ſo as to receive either edge of the ſhadow on its margin, all the fringes which had before been obſerved in the ſhadow on the wall immediately diſappeared, although the light inflected on the other ſide was allowed to retain its courſe, and although this light muſt have undergone any modification that the proximity of the other edge of the ſlip of card might have been capable of occaſioning. When the interpoſing ſcreen was more remote from the narrow card, it was neceſſary to plunge it more deeply into the ſhadow, in order to extinguiſh the parallel lines; for here the light, diffracted from the edge of the object, had entered further into the ſhadow, in its way towards the fringes. Nor was it for want of a ſufficient intenſity of light, that one of the two portions was incapable of producing the fringes alone; for, when they were both uninterrupted, the lines appeared, even if the intenſity was reduced to one-tenth or one-twentieth.

(To be continued.)

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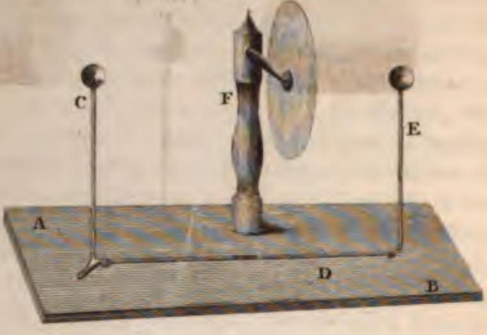
Experiments on heat by Count Rumford.

Fig. 2.



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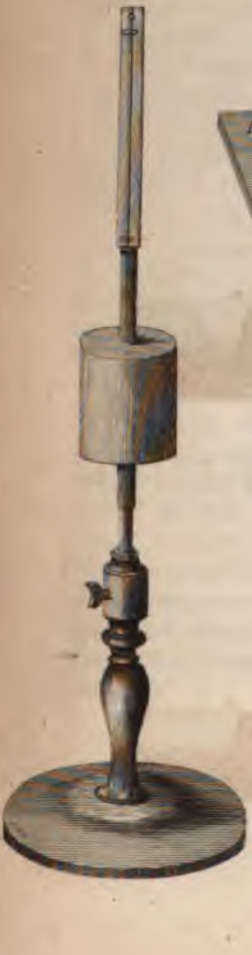
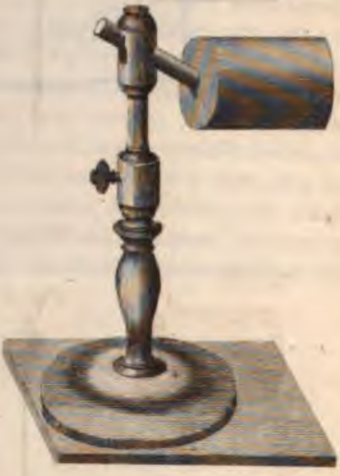


Fig. 3.



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Experiments on heat by Count Rumford.

Fig. 4.

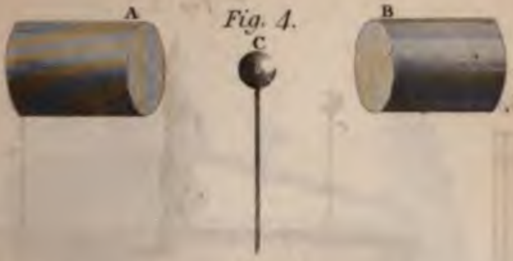


Fig. 5.

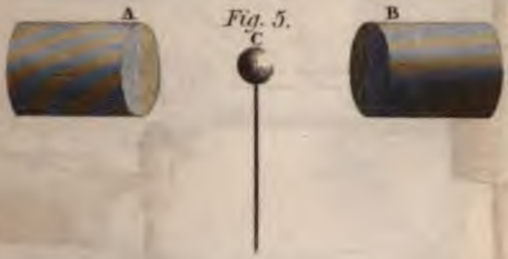
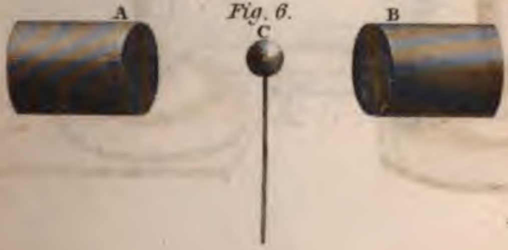


Fig. 6.



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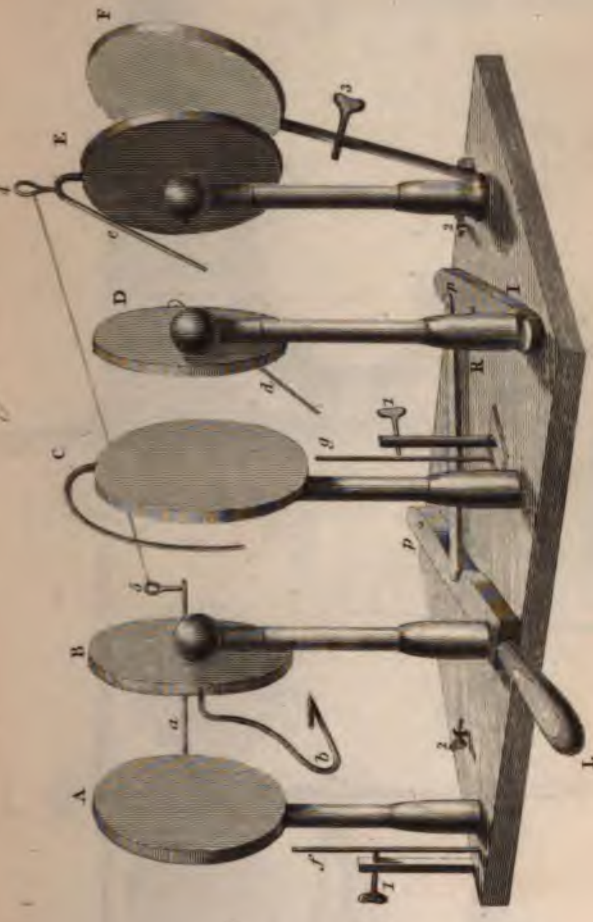

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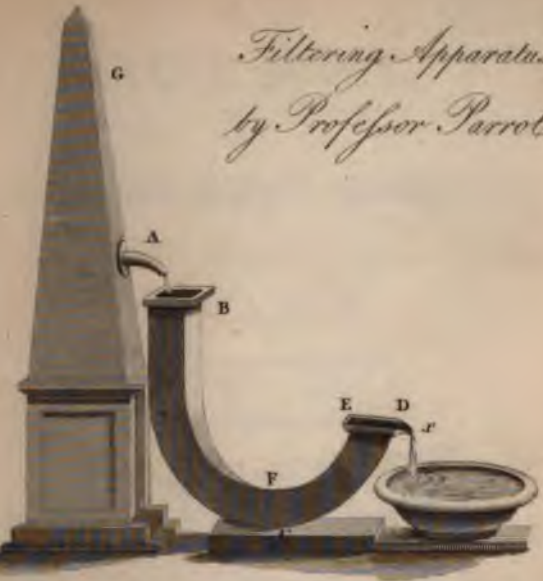
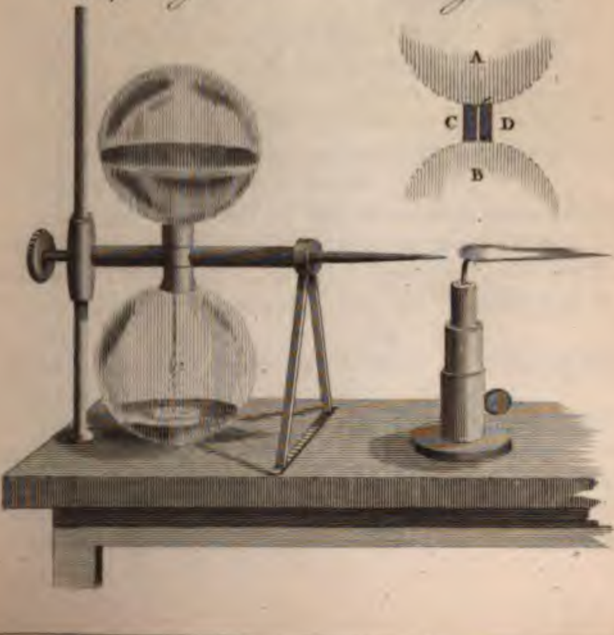


Filtering Apparatus, by Professor Parrot.

Blow-pipe by the Abbé Melograni.



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A

JOURNAL

OF

NATURAL PHILOSOPHY, CHEMISTRY,

AND

THE ARTS.

OCTOBER, 1804.

ARTICLE I.

Extract of a Letter from Count Apollos de Mouſſin Pouſhkin, to Charles Hatchett, Eſq. F. R. S. deſcribing his Method of preparing Malleable Platina. Communicated on the Requeſt of the Count, by CHARLES HATCHETT, Eſq. F. R. S. *and now firſt publiſhed.*

1. PRECIPITATE the platina from its ſolution by muriate of ammonia, and waſh the precipitate with a little cold water.

Purif. of platina.
1. Precip. the ſolution by mur. of amm.

2. Reduce it in a convenient crucible to the well-known ſpungy metallic texture, which waſh two or three times with boiling water to carry off any portion of ſaline matter which may have eſcaped the action of the fire.

2. Reduce to the ſpongy metal, and waſh.

3. Boil it for about half an hour in as much water mixed with one tenth part of muriatic acid as will cover the maſs to the depth of about half an inch in a convenient glaſs veſſel. This will carry off any quantity of iron that might ſtill exiſt in the metal.

3. Boil in weak mur. acid, to carry off all iron.

4. Decant the acid water, and edulcorate or ſtrongly ignite the platina.

4. Edulcorate and ignite;

5. To one part of this metal take two parts of mercury, and amalgamate in a glaſs or porphyry mortar. This amalgamation takes place very readily. The proper method of conducting it is to take about two drams of mercury to three drams of

5. Amalgamate with mercury;

platina; and amalgamate them together; and to this amalgam may be added alternate ſmall quantities of platina and mercury till the whole of the two metals are combined. Several pounds may be thus amalgamated in a few hours, and in the large way a proper mill might ſhorten the operation.

6. Mould the amalgam into pieces; which ſoon become ſolid.

6. After the amalgam is completely produced, it muſt be quickly moulded in bars or plates, or any other forms that may be preferred; taking care that theſe moulded pieces ſhould at leaſt be half an inch in thickneſs, and of a proper length to manage them afterwards in the fire; it is alſo requiſite that the moulds ſhould be perfectly even and ſmooth. Half an hour after the pieces are formed they begin to harden by the oxidation of the mercury, and change their brilliant metallic colour for a dull leaden one.

7. Expel the mercury, by ignition:

Laſtly, Heat the platina ſtrongly, and forge or laminate it.

To ſave the mercury on a large ſcale.

Remarks on this proceſs.

7. As ſoon as the pieces have acquired a proper degree of hardneſs to be handled without danger of breaking, which commonly takes place in a little more than an hour, place them in a proper furnace, and keep them ignited under a muffle or in a ſmall reverberatory. No other precaution is neceſſary in this operation but that of not breaking the pieces during their tranſport. The mercury flies off during the heat, and the platina remains perfectly ſolid; ſo that, after being ſtrongly ignited two or three times before the bellows, it may be forged or laminated in the ſame manner as gold or ſilver; care being taken, at the commencement of the forging or of paſſing it between rollers, not to apply too great a force till the metal has acquired all its denſity. It is almoſt ſuperfluous to add that in evaporating the mercury from large quantities of amalgam, a proper apparatus, ſuch as in the ſilver amalgamation, muſt be employed to receive the volatilized mercury; but for ſmall quantities, where the loſs of this metal is of no conſequence, the furnace muſt have a proper chimney to carry off the metallic vapours. When the platina comes out of the firſt fire its dimenſions are about two thirteenth parts ſmaller every way than the original amalgam from the mould. The whole of this operation ſeems to be governed by the preſſure of the atmoſphere and the laws of coheſive attraction: for the air is driven out from between the molecules of the platina, which by their ſolution in mercury are moſt probably in their primitive and conſequently uniform figure. It is very viſible and at the ſame time a very amuſing phenomenon to obſerve, (during the

proceſs of ignition, which is performed in four or five minutes) how the platina contracts every way into itſelf, as if preſſed by ſome external force.*

I have alſo lately obtained triple ſalts of muriate of platina with muriate of ponderous earth; and alſo with muriate of magneſia; and I ſtrongly ſuſpect that every other earth except the ſiliceous, and even the metals, are ſuſceptible of ſuch triple combinations. I have likewiſe obtained a very beautiful ſalt of platina by the combination of ſoda and platina with the muriatic acid; a combination which Bergman and ſeveral other chemiſts deny. The beſt manner of obtaining it is by diſſolving the platina in nitrous acid, to which, for that purpoſe, two parts of muriate of ſoda and one of platina are added. The platina muſt be made in a retort with its receiver, and after about four fifths of the fluid have come over, the proceſs muſt be interrupted, and the whole left to cool in the ſand bath. The ſalt cryſtallizes in fine priſms, which are ſometimes four or five inches long, and either red brown, like titanium, yellow, like amber, or of a beautiful coquelicot colour, according to the purity of the platina. I encloſe here my addreſs during my abſence, and hope you will receive with indulgence the contents of this letter.

Triple muriatic ſalts of platina, with earths and with metals;

—beautiful ſalt with ſoda.

I am with great regard, Sir,
Your moſt humble and obedient ſervant,
COUNT APOLLOS MOUSSIN POUSHKIN.

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* In the Count's letter to Mr. Hatchett, requeſting him to publiſh the method in the text (communicated to Mr. H. ſome years ago) the following addition is given: (in French.)

"As ſoon as my amalgam of mercury is made, I compreſs the ſame in tubes of wood, by the preſſure of an iron ſcrew upon a cylinder of wood, adapted to the bore of the tube. This forces out the ſuperabundant mercury from the amalgam, and renders it ſolid. After two or three hours I burn upon the coals or in a crucible lined with charcoal, the ſheath in which the amalgam is contained, and urge the fire to a white heat; after which I take out the platina in a very ſolid ſtate, fit to be forged."

On

II.

On Pepper. By THOMAS THOMSON, *M. D. Communicated by the Author.*

Vegetable chemiſtry neglected.

NOTWITHSTANDING the great number of labourers who have engaged in the cultivation of chemiſtry, the field of that alluring ſcience is too extenſive to be fully occupied. While many ſubdiviſions are left entirely waſte, and others exhibit here and there only faint traces of improvement, ſome fortunate ſpots, either from their ſuppoſed importance, or from the influence of faſhion or accident, have been crowded with workmen, and cultivated with enthuſiaſtic eagerneſs. The mineral kingdom has probably engaged the excluſive attention of nearly two thirds of the whole body of practical chemiſts: The animal kingdom, in conſequence of the intimate connection between chemiſtry and medicine, has enjoyed a conſiderable ſhare of cultivation; but the vegetable kingdom, though ſurely not inferior in importance, and apparently rather more alluring, has till lately been greatly neglected, at leaſt in Britain.

Reaſons why this branch muſt be always leſs accurate than mineral chemiſtry.

Indeed it is not likely that vegetable chemiſtry will ever arrive at the preciſion which we have a right to look for in the analyſis of minerals. The conſtituents of the latter ſeem ſcarcely ſuſceptible of altering their ſtate; but thoſe of vegetables run progreſſively through a regular ſuite of changes.—Thus the ſubſtance, which in the embryo ear of corn poſſeſſes the properties of *mucilage*, appears in the ripened grain under the form of *ſtarch*. Between theſe two extremes there exiſts an indefinite number of intermediate ſtates, through all of which the vegetable matter ſucceſſively runs. The rapidity and completion of theſe changes depend upon a multitude of circumſtances. In no caſe can they proceed exactly in the ſame order and at the ſame rate unleſs all the circumſtances tally. Without this the conſtituents of two vegetables even of the ſame ſpecies, cannot be in the ſame ſtate, and of courſe the analyſis of each, though conducted with the moſt perfect accuracy, will by no means exhibit an exact coincidence.

Changes in vegetable matter after life; and by reagents.

But even ſuppoſing the conſtituents of two vegetable bodies in every reſpect the ſame, ſtill the analyſis of each may lead to different reſults. Vegetable ſubſtances not only paſs through a ſuit of changes while they conſtitute a part of the living plant, but many of them are ſtill ſuſceptible of continuing the ſuit even after they are ſeparated from the parent that produced them. Thus *gluten* when kept moiſt runs into *cheeſe*; *oil* when long expoſed to the ſun and air hardens into *wax* or *reſin*, and the milky juices of plants into *gum-reſins*. Our analyſis frequently accelerates or occaſions theſe changes, and even produces others altogether new. Hence the *principles* which we extract from vegetable bodies are not always the conſtituents of theſe bodies; but a new ſet of principles formed during the analyſis, and of courſe varying according to the nature and circumſtances of the experimental inveſtigation. Hence we are ſeldom able to form again the old vegetable compound by uniting together all the ingredients which we have extracted from it.

Complication of principles.

Theſe difficulties increaſe with the complicated nature of the vegetable body; for the greater the number of conſtituents is, the more liable are they to undergo alteration during an analyſis. Indeed ſome vegetable principles ſeem incapable of exiſting except in combination, and are decompoſed or new modified the inſtant we attempt to ſeparate them.

Looſe nomenclature of vegetables.

The variety of ſtates in which the vegetable principles ſucceſſively exiſt, together with the difficulty of examining their properties without altering their nature, has rendered it neceſſary for chemiſts to apply the names of them with greater latitude than is uſual in other departments of the ſcience. A general reſemblance, indeed, in the moſt ſtriking properties, ſeems to have been thought ſufficient to entitle vegetable principles to the ſame name. If we examine all the bodies termed *gum*, we ſhall find them running on the one hand into *ſtarch*, and on the other into *ſugar*; and forming a pretty long ſeries, having ſugar and ſtarch at the two extremities: no two of the ſubſtances conſtituting the ſeries are exactly the ſame; but the ſame name is uſually applied to all thoſe that have a general reſemblance. This will be eaſily ſeen by the following table, in which I have ſubdivided the ſeries into five genera.

Inſtances in ſugar, gum, ſtarch.

Genus I. Sugar - -	1. COMMON SUGAR 2. Sugar of grapes 3. Sugar of beet.

- II. Sarcocoll
  1. Manna
  2. Liquorice
  3. Common ſarcocoll
  4. Saccharine of malt.
- III. Gum
  1. Mucilage of roots
  2. Cherry-tree gum
  3. GUM-ARABIC
  4. Gum of barks.
- IV. Gluten
  1. Gluten of barley
  2. Gluten of wheat
  3. Caſeous principle.
- V. Starch
  1. Gum tragacanth
  2. Gum of lichens.
  3. Common ſtarch.

Theſe terms are generic.

The terms *gum, ſtarch, ſugar,* &c. in vegetable chemiſtry are not to be underſtood as the names of peculiar ſubſtances, nor even as *ſpecies;* but merely as *genera,* and nearly ſimilar to the terms metals, acids, alkalies, &c. in the other departments of chemiſtry.

Subject introduced: *Pepper.*

The preceding obſervations will ſerve, I truſt, as an apology for the imperfection of the following remarks on *pepper.* I offer them to the public not as an analyſis of that vegetable ſubſtance, but as an account of ſome of the properties of its moſt remarkable conſtituents.

Deſcription.

I. BLACK PEPPER is the fruit of an Eaſt Indian plant, the *piper nigrum,* and is too well known to require any particular deſcription. The outer coat of the pepper-corn is brown, and a good deal ſhrivelled. Its taſte is not nearly ſo pungent as the inner part; of courſe it contains leſs of the peculiar principle to which pepper owes its taſte and ſmell.

Maceration in cold water.

II. When pepper is macerated in cold water it does not loſe its ſhrivelled appearance; a proof that the corns are impregnated with an oily ſubſtance, which prevents them from abſorbing water. The liquid very ſoon acquires a fine deep reddiſh brown colour, but retains its tranſparency. *White pepper,* which is known to want the outer coat, communicates no ſuch colour to water; the colouring matter then muſt reſide in the outer coat.

The watery infuſion thus obtained poſſeſſes the ſmell and taſte which are peculiar to pepper. Like moſt other *extracts,* it has the property of giving a red colour to vegetable blues. A very great quantity of water ſucceſſively applied is neceſſary to exhauſt the pepper of its colouring matter: but the ſmell and taſte of pepper become leſs and leſs ſtrong in theſe infuſions, and at laſt altogether imperceptible, leaving the infuſions inſipid, or ſlightly ſweetiſh.

Peculiar *extractive* matter.

1. Theſe cold infuſions contain a peculiar *extractive* matter, which ſeems to reſide in the outer coat of the pepper-corn. In the firſt infuſions their matter is united to the ſubſtance, in which the taſte and ſmell of pepper reſide, and occaſions its ſolubility in water. In the laſt infuſions, if we judge from their appearance, it ſeems to be mixed with a mucelaginous ſubſtance.

Cold and hot aqueous infuſions treated with infuſ. of galls.—Precip. of *ſtarch*.

2. If we mix the infuſion of nut-galls with the cold infuſion of pepper, no ſenſible change is produced; but in a decoction of pepper, it produces a copious flaky precipitate. Hence we learn that there is a ſubſtance in pepper inſoluble in cold water, but ſeparated by means of boiling water. This ſubſtance, as ſhall be afterwards ſhown, is a ſpecies of *ſtarch*.

Maceration of pepper in alcohol and diſtillation leave *volatile oil*.

3. When pepper is macerated in alcohol it communicates a light yellowiſh green colour to the liquid, which becomes at the ſame time fully impregnated with the peculiar hot principle which characterizes pepper. By diſtilling this tincture in a retort, the alcohol is obtained colourleſs, but of a decidedly peppery flavour. Towards the end of the diſtillation, the liquid in the retort becomes muddy, and depoſites a greeniſh matter, part of which may be obſerved alſo trickling down the ſides of the receiver like drops of oil. The reſidual liquid is yellow, but nearly inſipid. This green matter is the ſubſtance to which pepper is indebted for its taſte and ſmell. Its properties are analogous to thoſe of the *volatile oils*.

4. Theſe three bodies, namely, *extractive*, *ſtarch* and *oil*, I conſider as the moſt important ingredients of the pepper-corn. Let us examine the properties of each, beginning with the oil, which is obviouſly the eſſential ingredient.

Characters of oil of pepper.

III. The colour of the oil of pepper is graſs green. When firſt obtained it is of the conſiſtence of turpentine, but it gradually hardens by expoſure to the air. When moderately heated, it gives out a liquid oil of a yellowiſh green colour, and leaves a ſolid maſs, ſimilar nearly to a reſin. When thrown into water, it ſinks to the bottom of that liquid.

Taſte and ſmell ſtrong, and that of the pepper-corn.

Its taſte is intolerably hot, and preciſely ſimilar to that of pepper. So is its ſmell.

Habitudes. Inflammable,

When heated to 100°, it ſoftens; it melts at 148°, evaporates a little above 212° in a white ſmoke, which ſmells like tobacco ſmoke, irritating the throat and exciting coughing. Evaporated to dryneſs on a glaſs plate, it leaves a yellow trace behind it. When ſuddenly heated, it boils violently, and the vapour burns with a clear white flame without any ſmoke.

Volatile by ſtrong heat.

It gives a greaſy ſtain of a green colour to paper. At 500° the greaſy appearance is removed, but the green mark ſtill continues, unleſs the heat be ſufficient to char the paper. Hence I think it follows, that the colouring matter of this oil is a ſubſtance entirely diſtinct from the oil itſelf.

Soluble in alcohol and in ether, but not in water, &c.

The oil of pepper is inſoluble in water; alcohol and ether diſſolve it readily; the ſolution is light green: alcohol holding it in ſolution, acquires a very fragrant odour, preciſely ſimilar to that of oil of lavender. When water is added to this ſolution, the whole becomes milky, and paſſes in that ſtate through the filter. On ſtanding ſome weeks, light green flakes ſubſide, but the milky opacity is permanent.

Action of alkalis.

Alkalies have no ſenſible action on this oil while cold. When thrown into liquid potaſh it ſwims on the ſurface. If the liquid be heated, the oil becomes brown and acquires greater conſiſtence. At the ſame time it evaporates partially, diffuſing around the peculiar odour of pepper.

— of nitric acid,

Nitric acid diſſolves it with efferveſcence, the ſolution is yellowiſh brown, and a waxy matter ſwims on the ſurface. This acid acts in the ſame manner on other volatile oils.

— and of ox. mur. acid.

The oxygenized muriatic acid deſtroys the green colour and makes it yellowiſh white. But it ſtill retains its former taſte.

It is a vol. oil, with col. matter and reſin.

Theſe properties are ſufficient to authorize us to refer this green matter to the genus of volatile oils It is not however a pure oil, for beſides the colouring matter already mentioned, it obviouſly contains a ſubſtance which approaches to the genus of reſins in its properties.

Extractive of pepper.

IV. The EXTRACTIVE of pepper is procured by macerating the pepper-corns in cold water. Like the other ſpecies of this difficult genus of vegetable principles, it is ſcarce poſſible to obtain it in a ſtate of abſolute purity. But if we macerate the pepper-corns whole in ſucceſſive portions of water, till the liquid loſes the peppery flavour, and then pour on them freſh water, we obtain an infuſion which I believe holds in ſolution ſcarcely any thing but extractive. If this infuſion be evaporated ſlowly in a ſteam heat, it leaves a brown reſiduum, which I conſider as extractive of ſtarch not far from purity.

Aq. ſolution by evap. leaves ſtarch.

1. The infuſion is inſipid or ſlightly ſweetiſh, and of a fine reddiſh brown colour. As the evaporation advances, the colour deepens, and the liquid acquires an acrid flavour, ſimilar to that of ſcorched vegetable matter. This flavour, which indicates a commencement of decompoſition, is evolved at a very moderate temperature.

2. If the evaporation be conducted rapidly the extractive is opaque, dark brown, and has a perceptible taſte, and deliqueſces or at leaſt attracts moiſture when firſt expoſed to the atmoſphere. But by very ſlow evaporation I have obtained it in fine ſemi-tranſparent brown ſcales, which are inſipid, brittle, and not altered by expoſure to the air.

Soluble in water, but not in alcohol.

3. This extractive diſſolves readily in water, but not in alcohol. If it be repeatedly diſſolved in water, and the ſolutions evaporated to dryneſs, a ſmall portion of it becomes inſoluble in that liquid; but the greater part continues ſoluble after the proceſs has been repeated occaſionally even for months.

Precipit. by metallic ſalts, &c.

4. This extractive is precipitated from water by moſt of the metallic ſalts and by ſeveral of the earthy ſolutions.

It is thrown down in brown flakes by lime-water ſtrontian water and alum. Barytes water deepens the colour, but occaſions no ſenſible precipitate; nor is any precipitate produced by ſilicated potaſs or the magneſian ſalts.

It is precipitated brown red by the nitro-muriate of gold, the nitrate of ſilver, mercury, lead and biſmuth, and by the muriate of tin and antimony. When mixed with the infuſion of litmus, the colour becomes red. Ammonia reſtores the original colour, and at the ſame time throws down a copious blue lake.

—by ox. mur. acid.

5. When a current of oxymuriatic acid is paſſed through the infuſion of pepper, the brown colour is ſpeedily converted into a pale yellow, and the extractive precipitates in white flakes. The eaſieſt method of making this experiment is the following: Put a quantity of the hyperoxymuriate of potaſh into a ſmall retort, having a long neck (or a flaſk provided with a bent glaſs tube ground into it) and pour upon it a por-

tion of muriatic acid. Let the infuſion of pepper be put into a tall glaſs veſſel, and plunge the beak of the retort to the bottom of it. A current of oxymuriatic acid is diſengaged from the ſalt, and paſſes in bubbles through the infuſion for a conſiderable time.

When ammonia is added to the infuſion thus made white, the original red colour is reſtored. The white flakes precipitated by oxymuriatic acid gas are inſoluble in cold water.

Decoction of pepper affords with *tan*, a precip. ſoluble in hot, but not in cold water.

V. That pepper contains a ſpecies of ſtarch, I conclude from the following experiment, which I have frequently repeated. When the decoction of pepper is mixed with the infuſion of nut-galls, a copious precipitate falls in reddiſh brown flakes. If this liquid be heated to the temperature of about 120°, the precipitate is re-diſſolved, but appears again when the ſolution cools. Now the only ſubſtance which poſſeſſes the property of forming with tan, a precipitate nearly inſoluble in cold water, but very ſoluble in hot water, is ſtarch. Tan indeed throws down gluten, but the precipitate is not re-diſſolved in the application of heat. It throws down caoutchouc and ſome of the gum-reſins, but the precipitate is ſcanty and probably owing to the extraneous matter. The precipitate which it forms with gelatine and albumen cannot even by the moſt careleſs obſerver, be confounded with the compound of ſtarch and tan. But beſides this property, which I conſider as characteriſtic, the ſtarch of pepper, agrees with common ſtarch in the phenomena which it exhibits with the different chemical reagents.

This property is characteriſtic of ſtarch.

Phenomena of ſtarch with reagents.

As theſe phenomena have not yet been detailed by chemical writers, it may be neceſſary to give a ſhort ſketch of them in this place.

Starch not eaſily decompoſed.

1. Starch is one of thoſe vegetable bodies that are leaſt liable to decompoſition. It conſtitutes one of the moſt important articles of food, and acts an important part in the production of fermented liquors. The obvious properties of common ſtarch are too well known to require any deſcription.

Not ſoluble in alcohol, ether, or water.

2. Neither alcohol nor ether nor water are capable of diſſolving it. The laſt liquid when aſſiſted by a boiling heat, readily unites with it, and forms a kind of jelly, which may be diffuſed through boiling water; but when the mixture is allowed to ſtand a ſufficient time, the ſtarch ſlowly precipitates to the bottom. By drying the compound of ſtarch and water, a brittle

Forms a jelly with hot water which is little changed.

a brittle ſubſtance is obtained, differing in appearance from common ſtarch, but exhibiting nearly the ſame properties with re-agents. The apparent difference is probably owing to a portion of water remaining united to the ſtarch.

—Completely ſoluble in hot infuſ. of galls, and precip. by cold.

3. When ſtarch is triturated with the hot infuſion of nut-galls, a complete ſolution is effected. The ſolution is tranſparent and rather lighter coloured than the infuſion of galls. When cold it becomes opaque, and a copious curdy precipitate falls.

The infuſion of galls as prepared was *tan*.

The infuſion of nut-galls, which I am accuſtomed to employ in all my experiments, except when the contrary is expreſsly mentioned, is made by boiling together one part of galls in coarſe powder and two parts of water in a glaſs retort. When cold, the liquid part is decanted into a glaſs phial. It is at firſt muddy, and opake; but on ſtanding, a ſediment falls, and a tranſparent liquid remains of a deep brown colour, which conſtitutes my infuſion. An ounce meaſure of this infuſion, when evaporated to dryneſs in a glaſs veſſel placed on a tin-plate box, heated by ſteam, leaves a brown reſidue, which weighs 68 grains. This reſidue conſiſts chiefly of tan; for the greateſt part of the extractive gradually ſeparates from the infuſion in the ſtate of a brown, tough, imperfectly ſoluble membrane. Neither *extractive* nor pure *gallic acid* has any effect upon the decoction of ſtarch. Hence the precipitate is obviouſly produced by the ſole action of tan upon the ſtarch.

Experiment of the union of ſtarch and tan.

4. Twenty-four grains of ſtarch were triturated with half an ounce meaſure of the infuſion of galls, and mixed with about five ounces of hot water. A complete ſolution took place; but on cooling, the liquid became opaque, and a precipitate fell, which dried by a ſteam heat, weighed 35 grains. The reſidual liquor had a light yellow colour and an aſtringent taſte. When evaporated to dryneſs, it left a reſiduum that weighed 17 grains. This reſiduum contained ſtarch, for it was not completely ſoluble in alcohol. In this experiment ſome loſs muſt have been ſuſtained during the trituration. For the ſolid matter obtained weighed only 52 grains; or ſix grains leſs than the 24 grains of ſtarch and the 34 grains of ſolid matter in the infuſion of galls. The following is more to be depended on: After various trials, I found that ſtarch and tan are capable of uniting in different proportions. But the precipitate is leaſt ſoluble when ¼ oz. meaſure of infuſion of galls is uſed for every 24 grains of ſtarch.—I took 24 grains of ſtarch, boiled

them in a flaſk with five ounces of water, and then added ¼ oz. meaſure of the infuſion of galls. On cooling, a copious precipitate fell, and the liquid remained only faintly coloured. The precipitate dried in a ſteam heat weighed 31 grains, and the reſidual liquid left 11 grains of reſidue. The whole amounted to 42 grains; which is very nearly equal to the 24 grains of ſtarch and 17 of tan employed; the compound in this caſe conſiſts of

58.5 ſtarch, or nearly 3 ſtarch
41.5 tan - - - - 2 tan
100.0 5

The compound of ſtarch and tan is of a light browniſh yellow colour, ſemi-tranſparent and brittle, and has a good deal of reſemblance to common ſarcocoll. Its taſte is aſtringent; it ſeels glutinous between the teeth, like gum. It is very imperfectly ſoluble in cold water, but hot water diſſolves it abundantly. Alcohol digeſted on it acquires a brown colour; but is incapable of ſeparating the whole of the tan from the ſtarch. When heated, it froths, ſwells and melts, and then burns with a clear flame, leaving like ſtarch a ſmall portion of white aſhes behind it.

Infuſion of ſtarch treated with earths.—

5. To aſcertain the effect of the earths and metallic oxides on ſtarch, an infuſion of it was formed by triturating 24 grains of ſtarch with 4½ ounces of water, and then boiling the mixture for ſome time. The decoction thus formed, is nearly tranſparent, and of a ſlight opal colour. When ſet aſide, at leaſt a month elapſes before the ſtarch begins to ſubſide.

When lime water is mixed with this decoction, no change is produced; neither is any perceptible alteration occaſioned by ſtrontian water; but barytes water throws down a copious white flaky precipitate. This precipitate is re-diſſolved by muriatic acid, but appears again on ſtanding unleſs a conſiderable exceſs of acid has been added. Yet muriate of barytes occaſions no change in the decoction of ſtarch.

—with metallic ſalts: no effect.

No metallic ſalt ſeems to have the effect of throwing down ſtarch from its decoction. The following were tried:

Nitro-muriate of gold, platinum;
Nitrate of ſilver, mercury, lead;
Muriate of tin, acetite of lead;
Salts of copper, iron, zinc;
Ammoniated nickel and cobalt.

6. When potaſs is triturated with ſtarch, and a ſmall quantity of water added, the whole aſſumes on ſtanding the appearance of a ſemi-tranſparent jelly. On adding water, an opal coloured ſolution is obtained, from which the ſtarch is readily thrown down by an acid. When muriatic acid is employed, a peculiar aromatic odour is exhaled.

When the infuſion of galls is dropt into the ſolution of ſtarch in potaſs, a yellowiſh white precipitate appears, but is immediately re-diſſolved, and the liquid remains opake and of a dark brown colour. On adding muriatic acid, a copious precipitate falls, reſembling the compound of ſtarch and tan.—Nitric acid occaſions no precipitate, neither does ammonia.

The decoction of ſtarch is neither altered by potaſh, carbonate of potaſh, nor ammonia.

Treatment of ſtarch with potaſh. Solution. Separation by acids. The infuſion of ſtarch not affected by alkalies. Starch diſſolved by the ancient mineral acids. Phenomena.

7. Sulphuric acid diſſolves ſtarch, and abundance of charcoal is precipitated. Diluted ſulphuric acid, when aſſiſted by heat, diſſolves it without decompoſition. Sulphureous acid has no effect upon it.

Diluted nitric acid firſt reduces ſtarch to powder, and then diſſolves it, with the exception of ſome waxy matter, which ſwims on the ſurface. During the ſolution, ſome nitrous gas is exhaled.

Strong muriatic acid diſſolves ſtarch ſlowly, and without efferveſcence. When the ſtarch does not exceed $\frac{1}{20}$ of the acid, the ſolution is colourleſs and tranſparent, but if we continue to add ſtarch, a brown colour ſoon appears, and the acid loſes a portion of its liquidity. Its peculiar ſmell is deſtroyed, and replaced by the odour which diſtinguiſhes corn mills.

Acetic acid does not diſſolve ſtarch.

Experiments with alcohol.

8. Alcohol ſeparates ſtarch in part from its decoction. Potaſh diſſolved in alcohol occaſions a copious white precipitate, which is re-diſſolved on adding a ſufficient quantity of water. Alcohol digeſted on ſulphuret of potaſs, occaſions a flaky precipitate in the decoction of ſtarch: this precipitate has ſometimes an orange colour.

Starch of pepper has the preceding properties. It is united to an oil in the pepper-corn.

Such are the properties of common ſtarch: the ſtarch of pepper poſſeſſes them, excepting only that the colour of its precipitates is peculiarly modified by the other ingredients of pepper, from which it is not poſſible to free the ſtarch completely.

A very conſiderable portion of the pepper-corn ſeems to conſiſt of ſtarch, chiefly united to the oil of pepper. When a pepper-corn is cut in two, we find it compoſed of two coats. The outermoſt brown ſhrivelled coat is eaſily ſeparated by ſteeping the pepper in water. The ſecond coat is much thinner and lighter coloured; it does not ſeparate by maceration. Beneath this coat is a thick zone of pale green matter, apparently ſtarch united to oil. This is ſucceeded by a thin yellow zone, ſeemingly of nearly the ſame compoſition. Within this is a ſmall ſpherical ſpace, ſometimes hollow, but moſt commonly filled with a ſoft white ſubſtance like the pith of trees. All the zones have the peppery taſte. The outer coat has the flavour, but little of the heat of pepper.

III.

Letter from the Abbé Buée *on Mr.* Romé de Lisle's *and the Abbé* Hauy's *Theories of Cryſtallography.*

(Concluded from p. 39.)

SYNTHESIS is grounded, as I mentioned, on the fact, that all well formed cryſtals are terminated by plain ſurfaces.

Primitive forms:

Secondary.

Since there exiſt primitive forms, there muſt alſo be ſecondary forms, for the one ſuppoſes the exiſtence of the other. The ſecondary forms are ſuch, that ſections can be made only parallel to the ſides of the primitive; and when the primitive has been produced by theſe ſections, the diviſion being continued the integrant particles are obtained.

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The mineralogical analyſis deſcends from the ſecondary to the primitive form, and from the latter to the integrant particle; juſt ſo the mineralogical ſyntheſis aſcends from the integrant particle to the primitive, and from thence to the ſecondary forms. A cryſtalline edifice is therefore raiſed by means of the integrant particles. What are the laws of this extraordinary architecture? By laws I mean the diſpoſition of the laminæ, not the means employed by nature to execute the curious ſtructure.

Laws muſt exiſt, 1ſt, for the formation of the primitive, and 2dly, for the conſtruction of the ſecondary form. The primitives are either ſimilar to their integrant particles, or they are not. If they are not, their forms muſt be parallelopipedons,

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pedons, and their laws of formation very ſimple; for there will be the ſame number of integrant particles in each row, as there are rows in each lamina, as there are laminæ in the primitive form. It is eaſy to conceive that all the joints perfectly coincide with each other, and form continued planes; neither will there be any vacuity left between the particles. If the primitive be not ſimilar to the integrant particle, then the ſimplicity of the former caſe diſappears. I have already ſtated that there are three forms of integrant particles; the tetraëdron, the triangular priſm, and the parallelopipedon. There are alſo ſix primitive forms; the parallelopipedon, the octaëdron, the tetraëdron, the regular hexaëdral priſm, the dodecaëdron bounded by rhombs all equal and ſimilar, and the dodecaëdron with triangular ſides and formed by two right pyramids united baſe to baſe. Of theſe ſix primitive forms there are only the parallelopipedon and the regular hexaëdral priſm that can exactly fill up a ſpace without leaving any vacuity. The integrant particles of the former are parallelopipedons; of the latter, triangular priſms. As to the other four primitive forms, their integrant particles are tetraëdrons. The dodecaëdron bounded by rhombs is produced by twenty-four ſimilar tetraëdrons without any vacuity between them; the octaëdron and tetraëdron are formed by tetraëdrons leaving octaëdral vacuities; and the dodecaëdron bounded by triangles, to be formed of tetraëdrons, muſt imply ſections parallel to more than ſix planes; which perfectly coincides with obſervation.

Theſe vacuities, whoſe exiſtence muſt be admitted in the integrant particles, as well as between thoſe particles when forming a primitive, give riſe to the following reflections:

When the elements of a ſubſtance are chemically combined, that ſubſtance is homogeneous. Let us ſuppoſe a cryſtal of ſuch a ſubſtance to be ſubdivided into ſmall parallelopipedons equal and ſimilar; as the ſubſtance is homogeneous, and theſe little parallelopipedons having no vacuities between them, it is evident the elements that compoſe them are equal in number and proportion. We will next ſuppoſe the cryſtals of this ſubſtance can be divided by ſections parallel to ſix planes. In that ſuppoſition, nineteen or twenty different ſpecies of parallelopipedons can be produced. Among theſe ſpecies ſome will be ſimilar, others not; but none of the ſpecies will be exactly

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actly parallel to each other. We will proceed on two ſimilar cryſtals of the ſame ſubſtance and equal in ſolidity, dividing the firſt into one ſpecies, the other into a different ſpecies, of parallelopipedons, equal in ſolidity but not in ſurface; and let the diviſion of each be puſhed to its laſt term. But as we are come by ſmooth ſections to parallelopipedons of different ſpecies, thoſe ſections have alſo produced their differences: but by ſuppoſition theſe parallelopipedons are the reſult of the laſt poſſible term of diviſion without deſtroying the chemical compoſition, and being equal in ſolidity, though not in ſurface, they cannot contain each other; therefore if their differences are not integrant parts of both, theſe differences muſt ceaſe to be homogeneous, and we come to a ſort of chemical decompoſition. It is true we cannot execute this exceſſive diviſion, but we can form a very correct idea of it. If the little parallelopipedons contain two ſorts of elements, their differences will alſo, but alſo in different proportions; and, ſir, if you will turn to Berthollet's *Reſearches on the Laws of Affinities*, you will ſee him in all his experiments proving, that however perfectly a chemical decompoſition may have been made, the reſults will always contain a certain portion of thoſe ſubſtances from which it was the object of the operation to ſeparate them. If theſe reflections, Sir, are well grounded, do they not give us hopes, and perhaps ſhow the poſſibility, of deſcending from the integrant particles to the conſtituent particles? This ſecond reſearch is of the ſame nature as the firſt. It is more than probable that the conſtituent particles themſelves are diviſible, having no determined figure, but are aggregations, ſubject to the ſame laws as the integrant particles. The object of the natural philoſopher is not to diſcover the forms of the ultimate particles, but to determine their reſpective poſitions; which, if ever they could be determined in the integrant particles and their component parts, the grand problem of chemical affinities would be fully ſolved; and ſhould ſuch ever be the caſe, to the Abbé Haüy's theory would be due the merit. The *Encyclopædia Britannica*, under the article *Chemiſtry*, in the Supplement, p. 396, ſays:

"This theory, to ſay no more of it, is, in point of ingenuity, inferior to few; and the mathematical ſkill and induſtry of its author are entitled to the greateſt applauſe.

"But

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"But what we conſider as the moſt important part of that philoſopher's labours, is the method which they point out of diſcovering the figure of the integrant particles of cryſtals; becauſe it may pave the way for calculating the affinities of bodies, which is certainly by far the moſt important part of chemiſtry. This part of the ſubject, therefore, deſerves to be inveſtigated with the greateſt care."

But I return to the point whence this digreſſion carried me, to the vacuities left between the integrant particles in the conſtruction of a primitive form. The Abbé conſiders them as filled either by the water of cryſtallization or by ſome other ſubſtance. It is not an admiſſible ſuppoſition that this other ſubſtance is compoſed of the ſame elements as the integrant particles, but in different proportions? At leaſt, ſuch is the concluſion I ſhould be tempted to draw after reading Berthollet's excellent Reſearches on Affinities.

I ſhall now proceed to the laws of formation in ſecondary cryſtals. It is eaſy to deduce them from theſe two facts: *viz.* 1ſt, That the ſides of the ſecondary cryſtals are planes; 2dly, That they divide by ſmooth ſections parallel to the ſides of their primitive form.

Let us take a rhomboid of carbonate of lime for example. If on one of the ſides of the rhomboid I wiſhed to raiſe a pyramid, I ſhould lay laminæ of rhomboidal particles upon each other. Theſe laminæ would decreaſe in ſurface until the laſt is reduced to a ſingle rhomboid. Thus the ſecond lamina contains fewer particles than the firſt, the third fewer than the ſecond, and ſo on. As the faces of theſe pyramids are always to be planes, the ſucceſſive decrements of the laminæ muſt be equal; that is to ſay, the ſecond lamina is leſs by one range in every direction than the firſt, and the third than the ſecond, &c. If the decrement is more rapid; that is to ſay, if two or three ranges are ſubtracted in the ſecond lamina, the ſame number will be ſubtracted from the third, and ſo on ſucceſſively till the pyramid is completed. As the ſections are to be ſmooth, the joints muſt form one continued plane; therefore the ranges and even the particles at the joints muſt not encroach on each other: hence it follows *that the number of ranges ſucceſſively ſubtracted from each lamina can never be incommenſurable*; that is to ſay, the decrement may be 1, 2, 3, 4, &c.; but never $\sqrt{2}$, $\sqrt{3}$, &c.

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Theſe are the decrements parallel to the edges, or, as the Abbé calls them, *decrements on the edges.* But they may take place in a parallel with the diagonal of the faces of the primitive; they are then called *decrements on the angles,* becauſe the diagonals are drawn from one angle to the oppoſite angle. This ſecond ſpecies of decrement follows the ſame laws as the firſt.

There is a third ſpecies, called by our author *intermediate decrements.* In this caſe they are neither parallel to the edges nor to the diagonals of the faces, but to intermediate lines, which if prolonged would interſect both the edges and diagonals, but otherwiſe they follow the ſame laws as the two firſt. It is a general law, therefore, *that in all caſes the laminæ decreaſe in arithmetical progreſſion, and its ratio or the number of ranges ſubtracted is always commenſurable.*

The particles of which the laminæ are compoſed are to be conſidered as parallelopipedons; not that the integrant particles always have this figure; but if they have it not, they muſt leave vacuities between them, and each vacuity being added to its correſponding particle, will complete the parallelopipedon. If this was not the caſe, the faces of the ſecondary cryſtals would not be planes, nor could they be ſplit ſmoothly in any direction. Theſe little parallelopipedons which compoſe the ſubtracted ranges are what I called above, after our author, *ſubtractive particles.*

I ſuppoſed the conſtruction of the ſecondary form only to take place on one of the faces of the rhomboid; but what was ſaid relative to that face is applicable to all the others. It is alſo to be remarked that different laws of decrement may affect the different faces; even further, different laws may ſucceſſively affect the ſame face. Hence a diverſity of forms ariſe ſcarcely credible to a perſon unacquainted with the doctrine of combinations. The Abbé Haüy has calculated, "that confining one's-ſelf to decrements by 1, 2, 3, or 4 ranges, and not taking intermediate or mixt decrements into account, the rhomboid is capable of 8,324,604 varieties of cryſtalline forms.

It is an important remark, that whatever may be the variety of form, the forms (in complete cryſtals) will always be ſymmetrical. There are two ſorts of ſymmetry, the perfect and imperfect. In the perfect, the right is ſymmetrical with the left, and the top with the bottom; but in the imperfect, the top

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top is not ſymmetrical with the bottom. This latter ſpecies of ſymmetry appears, by general obſervation, to be excluſively appropriated to cryſtals that become electrical by heat; that is to ſay, which being expoſed to the heat of the fire, or plunged into hot water, acquire the electric power. Theſe cryſtals, the tourmaline, for example, acquire a poſitive electricity on one ſide, while on the ſide diametrically oppoſite their electricity becomes negative; and all obſervations hitherto made give us reaſon to conclude that theſe ſides are never ſymmetrical, and are always produced by different or fewer laws of decrement. "Hence," ſays the Abbé, "by mere inſpection it is eaſy to point out which is the ſide that will give the poſitive and which the negative electricity." (Vol. i. p. 237.)

The aſtoniſhing variety in the cryſtalline forms leads us naturally to aſk, What can be the cauſe of this variety? This queſtion has not been treated by the Abbé: allow me, Sir, to ſubmit a few ideas on the ſubject for the opinion of mathematicians.

Firſt cauſes, I repeat, are not the object of this diſcuſſion. I ſtate the queſtion thus: Why does the ſame ſubject cryſtallize in ſuch a variety of forms, always ſymmetrical and always terminated by planes?

The ſolution of this queſtion ſeems to require three conditions: 1ſt, That the particles of the ſubſtance diſſolved in the fluid all leave the ſtate of reſt at the ſame inſtant, to form the cryſtal by their aggregation: 2dly, That, while theſe particles are in the act of drawing near to each other, no foreign power ſhall imprint on them any other motion than a common motion, whether it be in a ſtraight line, or rotary round their common centre of gravity: 3dly, That the particles all arrive at the ſtate of reſt at the ſame inſtant, which takes place when the act of cryſtallization is finiſhed. The ſecond condition is neceſſary, and infers the firſt and third. The natural conſequence of theſe conditions will be, that the aggregation of the particles will only take place conformably to a law acting equally on all of them, whatever may be the law.

Since they all leave the ſtate of reſt at the ſame inſtant, they are in equilibrio previous to that inſtant. Since they all arrive at the ſtate of reſt at the ſame inſtant, they are in equilibrio after that inſtant: but when particles that are acted upon by

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no other force than that which they exerciſe on each other, are in equilibrio, they are in the cloſeſt poſſible union that concomitant circumſtances will permit. If the particles were in equilibrio previous to their leaving the ſtate of reſt, ſomething muſt have obſtructed their approach. Let us ſuppoſe that *ſomething* to be the interpoſition of another ſubſtance, and that ſo long as the interpoſition remains equilibrium is maintained. But this can only be the caſe, in as much as the whole of the particles of the interpoſed ſubſtance are in equilibrio with the whole of the particles diſſolved and about to leave the ſtate of reſt, which in the future I ſhall call the *proper particles*. If by any cauſe which acts uniformly on the whole ſurface of the diſſolving fluid any of the interpoſed particles are ſubtracted, the proper particles muſt ceaſe to be in equilibrio. A ſtep toward aggregation will immediately take place, and the equilibrium will be reſtored. A further ſubtraction will produce a further ſtep toward aggregation, and a conſequent equilibrium; and theſe operations will be repeated ſo long as the cauſe of ſubtraction continues, and the longer its duration the larger will be the reſulting cryſtalline maſs. If the above mode of reaſoning be admitted, it will ſuffice to apply the laws of equilibrium to deduce the laws of cryſtalline forms. The laws of equilibrium to which I allude, are thoſe of the equilibrium of fluids, with certain modifications which ſhall hereafter be explained. According to theſe laws, that the preceding conditions may take place in the formation of a cryſtal, it will be neceſſary that they take place in the formation of each and every part of it, whatever may be the figure or the ſmallneſs of thoſe parts. They muſt alſo take place in thoſe laſt cryſtals which contain the leaſt poſſible number of particles; and as theſe particles are in equilibrio, and in the greateſt poſſible ſtate of proximity to each other which circumſtances will permit, it muſt follow, to fulfil all the conditions, that theſe particles form a ſymmetrical polyëdron. This peculiar diſpoſition of the cryſtalline particles conſtitutes the modification, to which I alluded, in the laws of the equilibrium of fluids; it being neceſſary in this caſe to take the number of cryſtalline particles into account, which is not the caſe when treating of the particles of a fluid. In a fluid, the particles and their reciprocal diſtances are ſuppoſed infinitely ſmall; but the cryſtalline particles and their diſtances to each other

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other muſt be ſuppoſed finite. This material difference will neceſſarily cauſe a difference between the forms of their aggregates. Thoſe formed with the particles of a fluid will be bounded by curved lines; the cryſtalline aggregates, on the contrary, will be terminated by ſtraight lines; and when theſe ſtraight lines are not too ſmall, the boundaries will be ſenſibly rectilinear.

To aſcertain what the power is that holds the particles in the ſtate of reſt, though not in cloſe contact, is not the queſtion; but the form of the polyëdrons which they produce. The cloſer adheſion of the particles to be obtained by the ſubtraction of caloric, ſufficiently demonſtrates that the particles are not in cloſe contact with each other, and the conſtancy of the cryſtalline forms equally proves that they are in equilibrio. We will now proceed to the conſtruction of a cryſtal with theſe cryſtalline particles. That the conſtancy of the form in the large cryſtal be preſerved, the particles muſt be in equilibrio. That the equilibrium be preſerved, the forces that ſolicit the particles to motion muſt mutually deſtroy each other. That the mutual deſtruction of thoſe forces be effected, theſe forces after having been decompoſed into other relatively parallel to three axes perpendicular to each other, and having a common point of interſection, muſt each meet in its direction another force equal and diametrically oppoſed to it. This will be obtained if the ſimilar particles are arranged on ſtraight lines parallel two and two at equal oppoſite diſtances from the common centre, and biſected by lines paſſing through that centre; but if the particles are thus arranged, they muſt produce ſymmetrical ſolids bounded by planes; and they are thus arranged: for if a foreign force, an exceſs of caloric for example, does not impede the free arrangement of the particles in the formation of the cryſtal, their exterior diſpoſition will follow as much as poſſible their interior arrangement; but their interior arrangement muſt be on ſtraight lines, or the cryſtal would ceaſe to be homogeneous; their exterior diſpoſition will therefore be on ſtraight lines.

As the circumſtances giving riſe to the approach of the particles may be in the higheſt degree variable, it muſt follow that the forms produced may be diverſified in the extreme. Such, Sir, is the anſwer I ſhould ſubmit for the ſolution of the queſtion propoſed.

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When ſpeaking of the approach of the proper particles, I ſaid that it might be occaſioned by the ſubtraction of certain interpoſed particles which obſtructed the approach of the proper particles. The former are generally water, caloric, or any fluid elaſtic or not. Their exit may perhaps make place for others, ſuch as light, electricity, &c. &c. But the eſſential point is, that whatever theſe particles may be, they are in perfect equilibrio with the proper particles, otherwiſe they would become perturbing forces. Hence it follows, that not only the integrant particles of the cryſtal, but all thoſe that are mixed with them, the chemical or component particles and even the vacuities, muſt follow the ſame laws. It alſo follows, that if each ſpecies of particle (even the chemical) that enters into the formation of the cryſtal be ſeparately conſidered, each ſpecies will have its diſtinct ſymmetrical and polyëdral form. The forms will penetrate each other, while the particles will not only not penetrate, but not even touch each other. All forms would ſtand in the ſame predicament as the regular octaëdron, which contains, as the Abbé Haüy has demonſtrated, ſix regular octaëdrons and eight regular tetraëdrons, each tetraëdron containing one octaëdron and four tetraëdrons. It will further follow, if the chemical elements can be looked upon as particles which are not in contact with each other, that we may from thence mathematically determine chemical affinities.

I have now, Sir, but one taſk left; to ſpeak of the application our author has made of algebra and geometry to cryſtallography. Many perſons complain of the difficulty neceſſarily reſulting from it in the ſtudy of mineralogy; and dare not engage in it, uncertain whether they will find a compenſation for their trouble. Our author has therefore adopted a double plan, and begins by expoſing his theory by a ſeries of reaſonings and arguments which will ſuffice to make the reader underſtand it, or any diſcoveries made in conſequence of it. He then expoſes the theory in the moſt correct of all languages —mathematical analyſis; by far the moſt intereſting, and the only means of making diſcoveries one's-ſelf: and who can be callous to the pleaſure of diſcovering an unknown truth? If the ſolution of a problem gives ſo much ſatisfaction, though the data be only imaginary, what muſt be the ſenſations of thoſe who are happy enough to ſolve problems whoſe data are ſet

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ſet by *Him* whom the greateſt of pagan philoſophers calls the *Eternal Geometrician?* This recalls reflections to my mind which I cannot ſuppreſs. Converſing one day with the Abbé Haüy, he was taking a curſory view of all the modern diſcoveries; when he could not help remarking, that there was not one of them but what furniſhed victorious arms to the cauſe of religion. My anſwer was, that in future the name of God would be as diſtinctly written on a cryſtal as it had hitherto been in the heavens. The obſervation of this moſt religious and ingenious man reminds me of the ſaying of lord Bacon: " A little philoſophy eſtranges us from religion, but a great deal reclaims us again." Even d'Alembert could not help ſaying, " An atheiſt in the Carteſian ſyſtem is a philoſopher miſtaken in the principles; but an atheiſt in the Newtonian ſyſtem is ſomething worſe, an inconſequent philoſopher."

But to return to the mathematical part of our author's theory: the branch of mathematics, and the manner in which he treats it, are almoſt new. The theory of polyëdrons had been nearly neglected by geometers, both on account of the difficulty to repreſent a polyëdron on a plane, and becauſe they did not feel the utility of the purſuit. Neverthleſs, ſtrange to ſay, all the regular figures that are to be found in one of the three kingdoms of nature are polyëdrons. In this point of view, the branch of mathematics illuſtrated by the Abbé becomes very intereſting; and it is not a little ſo, to ſee with what ingenuity he extricates himſelf from the difficulties he meets with in his reſearches. He forms all the polyëdrons, however complicated, of little equal rhomboids or parallelopipedons, and by that means he reduces the theories of every poſſible polyëdron to that of the rhomboid, which is extremely ſimplified by two very ſimple remarks: 1ſt, That in all equilateral rhomboids, whatever may be the ſpecies, their projection on a plane perpendicular to their axes will always be a regular hexagon: 2dly, That the axes will always be triſected by perpendiculars drawn from all the lateral ſolid angles. His theory has alſo led him to diſcover in a variety of cryſtals geometrical properties, which muſt be highly gratifying to geometers. But the great advantage to be derived from it is, that it enables us with the feweſt poſſible data to calculate the cryſtalline forms juſt as aſtronomers do the motions of the heavens,

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heavens. By the very means by which the latter determine the future motions of the heavens, the Abbé decides which forms are poſſible and which are impoſſible. It is thus by his ſimple and general law of cryſtallization, "the number of the ranges of the ſubtractive particles muſt always be a commenſurable quantity," that he has demonſtrated the regular dodecaëdron and the regular icoſaëdron to be impoſſible forms in mineralogy. As the immortal Newton, by having diſcovered the law of attraction to be "in the inverſe ratio of the ſquares of the diſtances," explained and calculated every thing in the vaſt regions of the firmament; ſo at the other extremity of the creation the Abbé Haüy, by means of a ſingle law which he has diſcovered, explains the irregularities and calculates thoſe problematic formations with which the mineral kingdom had hitherto aſtoniſhed the natural philoſopher.

Laws, Sir, that reſult from the ſtudy of nature, enjoy this ineſtimable advantage, that they always lead to *equations*; and it is only by the help of equations (expreſſed or underſtood) that queſtions can be ſolved which relate to objects that can be either counted or meaſured.

Of late, Sir, the word *nature* has been ſo much abuſed, that I muſt beg leave to ſtate the preciſe ſenſe in which I wiſh to be underſtood whenever I made uſe of that word in the courſe of this letter. The Abbé Haüy found it neceſſary to take a ſimilar precaution at the beginning of the excellent work *(Traité de Phyſique)* he has lately publiſhed. He ſays: "This word NATURE, ſo frequently in our mouths, can only be looked upon as an abridged expreſſion, either for the reſult of thoſe laws which the GREAT CREATOR has imprinted on the univerſe, or for that aggregate of beings the works of his hands. Nature, thus viewed in its true light, is no longer a ſubject of cold and ſterile ſpeculation. The ſtudy of its productions, of its phænomena, ceaſes to be a mere exerciſe of the mind; it moves the heart, and ſtrengthens the moral virtues in man, by awakening in his mind ſentiments of reſpect and admiration at the ſight of ſo many wonders bearing the viſible characters of infinite power and wiſdom."

With theſe ſentiments I remain, Sir, your's,

A. Q. BUEE.

July 13, 1804.

Obſervations

IV.

Obſervations on Mr. Gough's *Strictures on the Doctrine of Mixed Gaſes, &c. In a Letter from Mr.* J. Dalton.

To Mr. NICHOLSON.

SIR,

Introduction.

FROM the formidable manner in which Mr. Gough opens the campaign, I might expect him to bring a hoſt of *facts* and *arguments* to his aſſiſtance; the *facts*, however, he prudently keeps in reſerve, and it is to be feared ſeveral of the *arguments* too, as thoſe already drawn out are ſcarcely of force ſufficient to provoke reſiſtance.

Mr. Gough's firſt *argument* is, that the ſubject in diſpute is more properly denominated *hypotheſis* than *theory*. This is certainly not worth contending about.

Whether the hypotheſis be mechanical.

Mr. Gough's next *argument* is, that the hypotheſis is not mechanical; but as this cannot be made good, it ſeems, without a geometrical diſquiſition of length much exceeding an ordinary letter, the philoſophical world are to wait till the ſame ſhall appear in due courſe. I might therefore, for the preſent, wave any remarks on this head, as a long and equally abſtruſe defence might be expected. But as I think geometry has nothing to do in the buſineſs, and that all that can be ſaid effectually, either for or againſt the hypotheſis, being conſiſtent with mechanical principles, may be comprized in one ſhort paragraph, I ſhall diſcuſs the argument here.

Poſtulate. The parts of a gas repel each other, but not thoſe of other-gaſes.

Oxigen repels oxigen, but not azote: This is a poſtulatum; and being admitted, it follows, that if a meaſure of oxigen be put to one of azote, the oxigen finding it porous, muſt enter the pores, and *vice verſa*, till the two gaſes ſeverally making their way into the interſtices of the other, at laſt obtain a perfect equilibrium, and then preſs with equal force on all the ſurrounding bodies, and no longer preſs on each other. This is ſo plain and obvious an inference, and ſo little involves any mechanical conſideration, that I ſhould have juſtly incurred blame for inſulting my readers with the appearance of mathematical demonſtration in the caſe. As well might I have attempted, from the elements of Euclid, to demonſtrate to a cottager, that if he put a ſieve over his chimney the ſmoke would ſtill eſcape, though interruptedly; or to a chemiſt, that if he drilled holes in an exhauſted receiver, it would in time be completely filled with air.

On the action of aqueous vapour on water.

Mr. Gough next remarks upon my opinion, that the atmoſphere of aqueous vapour is ſufficient to prevent the ocean from eſcaping into the air, which he ſeems to think wonderful: Upon this I may remind him of another wonderful fact, which I conſider the ſame in effect; that is, the preſſure of like vapour on a cup of water in an exhauſted receiver, prevents the water from eſcaping out of the cup. When Mr. Gough ſhall explain how this fact is to be accounted for, I may avail myſelf of his explanation to apply to the other. He proceeds, however, to demonſtrate the impoſſibility of an aqueous atmoſphere; but inſtead of that, he demonſtrates *the impoſſibility of an atmoſphere of any kind preſſing on water, without at the ſame time forcing the water up into its pores.* Luckily, as Mr. Gough obſerves, the facts do not countenance the concluſion; and therefore, however rigid the demonſtration, it ſhews that the previous data are not correctly aſſumed. The conſideration of this ſubject is, notwithſtanding, an important one; it is more than a year ſince I urged Mr. Gough to pay attention to it, and to attempt a ſolution of the difficulty which perplexes all theories of the atmoſphere alike: I did it the rather, becauſe I thought him well qualified for a ſubject of this nature, where the aid of mathematical ſcience muſt be of ſubſervience. The ſubject has never been at all explained that I know of. I have made an attempt which has not yet been publiſhed, except in a lecture laſt winter; but I ſhall ſtill be glad to avail myſelf of any aſſiſtance I can obtain.

More particular ſtatement of the fact; and queſtion.

Suppoſing that in the loweſt ſtratum of an atmoſphere incumbent on water, there is *one* particle of gas for *one hundred* of water, (which may be the caſe, conſidering their relative denſities as *one* to a *thouſand*); *query*, How does the air diffuſe its preſſure over all the hundred particles equally, in ſuch ſort that no column of particles of water is forced up into the interſtices of the atmoſphere by the inequality of the preſſure? If Mr. Gough will explain this, either for his new theory or for any other, I will engage to remove all difficulty on this head which attaches to my hypotheſis.

Impediments afforded by one gas to the diffuſion of another.

Mr. Gough next wants a rigorous proof of the propoſition, that one gas affords an impediment to the motion of another, but that two ſuch fluids finally overcome their mutual obſtructions, and occupy the ſame ſpace in a ſtate of perfect independence. No one acquainted with the experimental part of pneumatic chemiſtry would have required proofs of ſuch facts, becauſe he daily experiences them. Take two phials filled with different gaſes; apply the mouth of one to that of the other for a few moments; upon withdrawing it, the two phials will be found to have interchanged very little of their contents. This is a moſt unqueſtionable proof that gaſes afford impediment to each other's motion; for, into a perfect vacuum the air ruſhes inſtantaneouſly. Again, let the phials remain in connection for a few minutes, or at moſt, hours, and they will be found to have both gaſes in the ſame proportion; and this ſtate will continue in perpetuity afterwards. That they are ultimately *independent* on each other, is ſufficiently marked by the circumſtance, that any ſubſtance having an affinity for one of them will withdraw it from the mixture, if it will take it alone.

Mr. Gough's new theory of mixed gaſes.

Mr. Gough proceeds to give a *new theory of mixed gaſes*: At the commencement of this controverſy he came forth to *defend* the all-ſufficiency of the old doctrine of the chemical union of water and air, and the homogenity of the atmoſphere, and to *attack* the new doctrine, which proſcribes chemical union in theſe inſtances, and places for its fundamental and diſtinguiſhing maxim, that "mixed gaſes neither attract nor repel one another:" It was therefore with no ſmall ſurpriſe that I found, upon his advances, that a *new theory* was ſtill requiſite; but this ſurpriſe became aſtoniſhment, when I found the preamble admitted the exiſtence of certain, "gaſes that neither attract nor repel each other." The only clear information I could obtain from this ſudden revolution was, that Mr. Gough uſes the terms *theory* and *hypotheſis* in a contrary ſenſe to what philoſophers in general do. When I brought my *hypotheſis* (as he calls it) forward, it was ſupported by an extenſive train of facts, the reſult of long and careful inveſtigation, none of which, that I know of, has ſince been controverted; I mean what Mr. Gough alludes to as a "number of probabilities of an experimental nature:" Whereas Mr. Gough's *theory* is propoſed without a ſingle a ſingle fact to corroborate it, merely to try how far it may be found to agree with facts already known. Leaving Mr. Gough to develope his theory, which I confeſs I do not comprehend, and therefore cannot follow him in the application, I proceed to his concluding remarks on the law of affinity, which connects water with the different gaſes.

Water ſuſpended or diffuſed alike in all gaſes.

Sauſſure, to whom Mr. Gough refers, in the ninth chapter of his ſecond eſſay inſtructs us, that the *ſolvent powers* of common air, carbonic acid, and hydrogen, are the ſame as far as his experience goes. The reſults of Kirwan's experiments I am unacquainted with. Clement and Deſorme, in the forty-ſecond volume of the *Annales de Chimie* for 1802, have clearly ſhewn, that all gaſes take up the ſame quantities of water, alcohol, and ether, in like circumſtances; and I think I have ſhewn, that theſe quantities are preciſely the ſame as a torricelliam vacuum of the ſame capacity takes up: So that unleſs Mr. Gough can hold out ſome further encouragement to the reſumption of the enquiry, it ſeems hardly likely that any one will undertake to inveſtigate the *diverſities* in the law of affinity between gaſes and water, when there does not appear a ſingle fact that points out any affinity at all in the caſe.

I am your's, &c.

J. DALTON.

Mancheſter, Sept. 8, 1804.

V.

Account of the Striking Part of an Eight-Day Clock. By Mr. John Prior, *of Neſsfield, Yorkſhire. Communicated to the Society of Arts.*[*]

To CHARLES TAYLOR.

SIR,

New ſtriking part of a clock.

I HAVE taken the liberty of ſending to you a new ſtriking-part for an eight-day clock, of my own invention and workmanſhip, which I finiſhed laſt April; * * * * * *. I beg

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[*] Extracted from their Tranſactions, 1803. A reward of thirty guineas was given by the Society, who have a model of the ſame.

New ſtriking part of a clock.

leave to inform the Society of ſome of the advantages ariſing from this new invention.

Firſt, it conſiſts in a wheel and fly, with ſix turns of a ſpiral line, cut upon the wheel, for the purpoſe of counting the hours. The pins below this ſpiral elevate the hammer, and thoſe above are for the uſe of the detent.

This ſingle wheel ſerves the purpoſe of count wheel, pin wheel, detent wheel, and the fly wheel, and has ſix revolutions in ſtriking the twelve hours.

Permit me to ſuppoſe a train of wheels and pinions, uſed in other ſtriking parts, to be made without error, and that the wheels and pinions would turn each other without ſhake or play. Allowing the above ſuppoſition to be true, which every mechanic knows it is not, my ſtriking part will be found ſix times ſuperior to others, in ſtriking the hours 1, 2, 5, 7, 10, 11, and twelve times, in ſtriking 4, 6, 8, and eighteen times, in ſtriking 3, 9, and 12.

I have deſignedly made a defect in the model herewith ſent, in ſtriking 2 and 3 o'clock, to ſhew that what I have now advanced will, upon the trial, be found to be true.

In ſtriking 2, I have purpoſely made an imperfection, equal to the ſpace of three teeth of the wheel; and, in ſtriking 3, an imperfection of nine or ten teeth; and yet both theſe hours are ſtruck perfectly correct.

The flys in clocks turn round, at a mean, about ſixty times for every knock of the hammer, but mine turns round only three times for the ſame purpoſe; and ſuppoſe the pivots were of equal diameters, the influence of oil on them would be as the number of revolutions in each. *

It would be better for clocks if they gave no warning at all, but the ſnail-piece to raiſe a weight ſomewhat ſimilar to the model now ſent for the inſpection of your reſpectable Society.

Reference to Mr. PRIOR's *Striking Part of his Clock.*

Plate VI. Fig. 1.

A. The large wheel, on the face of which are ſunk or cut the ſix turns of a ſpiral.

B. The ſingle worm ſcrew, which acts on the above wheel, and moves the fly C.

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* See Cummings's Elements.

water with which the metropolis is ſupplied contributes materially to the health and comfort of thoſe who uſe it, and in fact renders it purer than almoſt any known ſpring water, a very cheap and commodious apparatus for the purpoſe may be conſidered as an object of general utility. I therefore ſend you a ſection of a machine which I conſtructed ſeveral years ſince, and which any common carpenter can make for a very few ſhillings, which may be thoroughly cleanſed at any time, and which occupies very little room.

As you have already honoured ſeveral of my ſhort eſſays, publiſhed in other collections, with inſertion in your Journal, you will, I truſt, not be diſpleaſed that we ſhould become immediate correſpondents.

I am, Sir,
Your obedient Servant,
H. C. ENGLEFIELD.

P. S. I will not anſwer for it that the machine I ſend you has not been already introduced into uſe. If it has, you will of courſe ſuppreſs my letter; but if it has, Profeſſor Parrot's filtre is quite nugatory.

Arrangement for purifying water on a large ſcale.

A moſt excellent arrangement for the purification of river water on a large ſcale is mentioned in the writings of De Luc or De Sauſſure, but I cannot turn to the paſſage in their works. It was applied with complete ſucceſs by the inventor, to the ſtream which ſupplied a large town in Switzerland. The machine (if it may be ſo called) was as follows:

A is the upper ſurface of the ſtream to be purified, B the bottom. A ciſtern is ſunk of ſix or ſeven feet in depth, and of a proper breadth, divided by parallel partitions, alternately riſing above the ſurface level of the ſtream and open at the bottom, and level with the bed of the river, and cloſed at the bottom. It is obvious that the courſe of the water muſt be in the direction of the arrows, and in this repeated and ſlow aſcent and deſcent, all floating impurities will be left at the top, and the heavier mixtures will ſubſide. The ciſtern may be eaſily cleanſed, either by taking out the partitions, if it is on a ſmall ſcale, or by ſending perſons down between the walls, if it is built permanently for a great ſtream. Perhaps, indeed, a box having the partitions filled half the way up with ſand or gravel, may on this plan be the beſt of all filtres for domeſtic uſe.

Deſcription of the Filtering Apparatus, Plate V. *Fig.* 1.

Filtering apparatus deſcribed.

A B The exterior tube, two feet high and ſix inches ſquare within.

C D The interior tube, four inches ſquare at the top and three inches at the bottom. It reaches within about three inches of the bottom of the exterior tube, and is covered at the bottom with a coarſe linen tied round it. The uſe of this is to prevent the weight of the water from diſturbing the ſand. The upper end of this tube is formed into a funnel, for the convenience of filling it with water, and it reſts on the outer tube. E a ſpout for the exit of the filtered water.

Both tubes are filled with clean waſhed ſand up to the dotted line juſt below the ſpout. A bag for ſtopping the coarſer impurities may be adapted to the funnel. If this machine be placed under the cock of any common water ciſtern, which is opened juſt enough to ſupply the funnel without running over, it will require no attendance, and will very ſeldom want cleaning. It is obvious that every part of the machine, when the two tubes are taken aſunder is viſible to the eye, and eaſily reached by the hand. The ſand, when waſhed, will ſerve many times.

If inſtead of a funnel, a larger reſervoir of water at the top is uſed, which may ſometimes be convenient, it will be beſt to fill the upper part of the inner tube for a few inches with clean ſmall pebbles, as the pouring in water diſturbs the upper ſurface of the ſand.

It may be made either of wood or tin, but not of lead, for fear of impregnation. It is alſo evident that the rapidity of action of the filtre will be in a great degree regulated by the difference of level between the ſpout and the ſurface of the water in the funnel, and by ſupplying the funnel with a greater or leſſer ſtream, the machine may be made to act as quick or as ſlow as is wiſhed.

Whether charcoal would be advantageous in a filtering apparatus for water.

As the water which ſupplies the metropolis is often tainted with vegetable or animal ſubſtances putrified in it, it might be well worth while to try whether filling the inner tube with powdered charcoal might not tend to free the water filtred through it from the diſagreeable taſte and ſmell communicated by the cauſes above-mentioned. It will alſo be advantageous to place the receiving veſſel at ſome diſtance below the ſpout, that the ſtream may fall through as much air as it conveniently can.

Experiments

VIII.

Experiments on the Effects of Heat modified by Compreſſion, by SIR JAMES HALL, *Bart. Read in the Royal Society of Edinburgh, Auguſt* 30, 1804. *Communicated by the Author.*

Experiments of heat, modified by compreſſion.

I BEG leave to announce to this Society the reſult of a ſeries of experiments which have occupied my attention almoſt excluſively during ſeveral years. Theſe experiments relate to the effects of heat modified by compreſſion, and are intended to inveſtigate the peculiar and characteriſtic principle of the Huttonian theory.

Dr. Hutton's theory of mineral compounds naturally produced in this way.

Dr. Hutton, in common with many former geologiſts, has aſcribed the formation of all mineral ſubſtances chiefly to fire. But, according to him, the influence of this element has been very much modified by compreſſion, occaſioned by the weight and ſtrength of a vaſt ſuperincumbent maſs, which then preſſed upon what is now the ſurface of our globe. In this manner he has anticipated the natural objection to all igneous theories which muſt ariſe from a compariſon of various mineral ſubſtances with the products of fire in our furnaces; for he conceives that preſſure by repreſſing volatility would occaſion the preſence, in high temperatures, of many ſubſtances which eſcape in our fires, on a ſlight application of heat; and that theſe by their chemical relations would give riſe to a ſtate of things untried in any experiments hitherto publiſhed, but ſuch as to afford a ſatisfactory explanation of all the natural phenomena upon his hypotheſis, even of thoſe which are the moſt incompatible with the common action of fire.

The facts are that heat has ſo acted in our globe; but the effects have not been ſhewn by experiment.

The two fundamental *poſtulata* required in this theory, namely, the action of heat, and the preſence of a ſuperincumbent maſs are certainly allowable; ſince the volcanos furniſh us with a proof that internal fire does act occaſionally and in the irregular manner which this ſyſtem requires; and ſince the fragmented and perturbed ſtate of our ſtrata enables us to ſay with certainty that great changes have taken place, that enormous maſſes have been removed, and that what was once placed at a great depth is now highly elevated.

But a third *poſtulatum* is involved in this theory, which ſeems to be of more difficult admiſſion. Granting that heat did act on ſubſtances conſtrained by preſſure, would its action be modified? Would that modification be ſuch as is aſſumed in the Huttonian theory?

To theſe queſtions Dr. Hutton has replied by arguments founded on general analogy; and has reſted the proof of his hypotheſis on its agreement with the phenomena of nature. In that reſpect few philoſophical theories have been ſo fortunate; for its univerſal application to every department of the mineral kingdom, and its ſolution of all the difficulties, afford a concurrence of probabilities in its favour which preſs on the mind with almoſt irreſiſtible conviction. Still it muſt be owned that the baſis of the ſyſtem is hypothetical; and a wiſh has been expreſſed by every man of ſcience who has attended to the ſubject, that this baſis ſhould be ſubmitted to the teſt of experiment.

Dr. H. has endeavoured to eſtabliſh theſe effects from geological obſervations.

My object has been to accompliſh that end, and to bring this great queſtion to an *experimentum crucis*. By placing ſubſtances in the predicament aſſigned to them in the Huttonian theory, I have endeavoured to imitate the ſuppoſed proceſs of nature. In this attempt I have met with great and numerous difficulties, but I have at laſt ſucceeded beyond my original expectation, and have obtained reſults, which, if I am not greatly deceived, eſtabliſh as a law of chemiſtry the moſt paradoxical of Dr. Hutton's poſitions.

The author recurs to actual experiment.

My experiments ſhew, that when pounded carbonate of lime, produced by the trituration of chalk, of marble, of the ſhell of a fiſh, or of calcareous ſpar, after being rammed into a ſmall tube of porcelain, is expoſed in veſſels of ſufficient ſtrength and tightneſs to the heat of 21 or 22 of Wedgwood's pyrometer (that is to the heat in which pure ſilver melts,[*]) the

Carbonate of lime agglutinates (at 22° Wedgwood) in ſtrong cloſe veſſels, and becomes limeſtone.

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[*] I take this opportunity of mentioning that a very material error ſeems to prevail with reſpect to this point of the pyrometrical ſcale. The error is the more formidable that it has been introduced and ſanctioned by the higheſt authority poſſible in ſuch a caſe; I mean that of Mr. Wedgwood himſelf. In his account of the pyrometer he gives a table, expreſſing the effects produced at various points of temperature, and ſtates 28 as the melting heat of ſilver. Now it conſiſts with my knowledge that pure ſilver melts at 22. I learned the fact from Dr. Kennedy, and I have had occaſion to confirm the truth of it in numberleſs trials.

the carbonate ſhrinks upon itſelf and agglutinates into a firm maſs, which in point of hardneſs and ſpecific gravity approaches very near to common lime-ſtone, and ſometimes equals it, and which has frequently acquired the ſparkling fracture, the ſemi-tranſparency, the ſuſceptibility of poliſh, and the general aſpect of marble. The ſame reſult is obtained when a ſolid piece of chalk is treated in a ſimilar manner, and the chalk being previouſly meaſured in Wedgwood's gage, is found to contract during the action of heat three times more than the pyrometer pieces do in the ſame temperature. During the action of heat, the carbonate is found to have loſt very little of its weight; that loſs amounting in many caſes to leſs than one per cent. and in ſome experiments it has undergone no ſenſible loſs at all, or ſo very ſmall a one, that it may be neglected without fear of error. When thrown into an acid, this artificial limeſtone efferveſces violently as it diſſolves, the diſcharge of gas continuing whilſt the ſmalleſt atom of carbonate remains viſible.

Great contraction of the chalk.

Imperfect fuſion of the chalk.

I have been in poſſeſſion of this fact ſince the year 1801, and I long attempted in vain to carry the experiment farther, ſo as to accompliſh the fuſion of the carbonate. In one ſolitary and accidental inſtance, I had ſucceeded in obtaining it in a ſtate of real froth, which could not have been produced without previous liquefaction; but being unable to repeat this reſult, I was unwilling to publiſh it or any of the facts already ſtated, till I could do ſo in a more ſatisfactory manner. In the courſe of laſt winter, with the help of many improvements in my mode of operation, and of ſtronger apparatus, I at laſt acquired the power of performing repeatedly and even with tolerable certainty, what at firſt had been the effect of chance. In theſe experiments carbonate of lime has not only been ag-

Actual fuſion.

This obſervation relates to the pyrometer pieces ſold by the late Mr. Wedgwood, which were formed of a mixture of alumine with Corniſh porcelain clay. This ſet having been the only one ever diſtributed amongſt chemiſts, muſt certainly be looked upon as the ſtandard. Other ſets had previouſly been made by him of Corniſh clay alone, which had never been ſent abroad, or at leaſt only given to ſome friends. It is poſſible that the diſcordance alluded to in this note, may have been occaſioned by experiments made with thoſe firſt ſets, which may have poſſeſſed different properties from thoſe afterwards ſold.

glutinated, but actually fuſed; the ſubſtance ſinking upon itſelf with a round and gloſſy ſurface, and exhibiting every proof of a viſcid fluidity, ſimilar to that of melting ſealing wax. In general the fuſion has been accompanied with a ſlight ebullition, which has ſometimes changed the maſs to a kind of froth, and ſometimes has merely produced ſome ſcattered air bubbles. The whole externally and in its fracture ſhines much; this ſhine, ariſing in ſome caſes from numberleſs facettes of cryſtallization, and in others from a ſmooth and continued gloſs, like that of glaſs. In many ſpecimens the cryſtallization of newly formed ſpar is diſtinctly viſible; the cryſtalline maſs conſiſting of parallel plates, which reflect together with one gloſs. Some of theſe are diſcernible by the naked eye, though in general to ſee them well we require the help of a lens. As ſoon as the carbonate becomes ſoft, it begins to act powerfully on the tube of porcelain (generally formed of pure Corniſh clay) in which it is confined; the compound ſhewing itſelf to be much more fuſible than the pure carbonate. It penetrates the minuteſt crevices, and ſpreads along the cup to a conſiderable diſtance from the point of contact between the carbonate and the tube; its termination being marked by a black line, the cauſe of which I have not diſcovered. Previous to this ſtage of fuſion no action whatever ſeems to take place between the carbonate and the porcelain, the former receiving from the latter an accurate impreſſion of its ſhape, acquired doubtleſs when the powder was rammed into the tube. In this caſe the carbonate remains quite looſe, and is often heard to rattle before the veſſel is opened. Where pounded ſilex has been rammed into the tube in contact with the carbonate, an union has ſometimes taken place, producing a ſubſtance having ſomewhat the appearance of chalcedony, but which ſhews evident proof of fuſion, it having flowed ſo as to form little ſtalactites and ſtalagmites. This ſubſtance effervesces feebly in acid, in ſome caſes it leaves a ſemi-tranſparent cloud of undiſſolved matter, in others diſſolves entirely the ſolution, yielding a jelly when evaporated to a certain pitch. This affords proof of a real union between the carbonate and the ſilex.

Sparry cryſtals in the fuſed carbonate.

Action of the carbonate on the clay veſſel.

Union of the carbonate and ſilex.

In all the experiments alluded to in this paper, the veſſels have been expoſed to a violent expanſive force, by which a great number of them have been deſtroyed, and the experiments have often been loſt or reſults obtained only of partial ſucceſs,

When the veſſels have failed, the carbonate has ſhewn different habitudes from loſs of acid.

ſucceſs. But theſe have frequently been of value, by bringing into view important collateral facts. Thus I have found that under certain circumſtances, a partial calcination has taken place by the ſeparation of ſome of the carbonic acid from the lime, though enough ſtill remained to preſerve many of the leading properties of a carbonate. When a loſs is ſuſtained, amounting only to two or three, or even four per cent. I find the ſubſtance ſtill ſuſceptible of agglutination and fuſion, but its fuſibility is greatly diminiſhed, a heat of 40 or 50 being required to accompliſh what would have been done in 22 or 25, had the earth continued to be completely ſaturated with carbonic acid, and the carbonate thus obtained is apt to fall to decay by attracting moiſture from the air. Theſe differences afford a good illuſtration of the influence exerted by the acid as a flux on the earth.

—being leſs fuſible, &c.

An apparatus reſembling that of Count Rumford,

Having thus aſcertained the fuſibility of the carbonate under preſſure of indefinite amount, I became deſirous of aſſigning its limits, and of diſcovering the leaſt force neceſſary for this purpoſe. In this view, in addition to my other devices, I followed thoſe uſed by Count Rumford in trying the exploſive ſtrength of gun-powder.*

—and regulated.

By means of a great weight preſſing upon a ſmall opening, and regulated by a counterpoiſe adjuſted at pleaſure, I was able to conſtrain the carbonate to any given amount. In this manner I found that the preſſure of 80 atmoſpheres, anſwering nearly to half a mile of ſea in depth, was requiſite to produce any effect of compreſſion on the carbonate of lime, and that to execute the buſineſs well required a force four or five times greater.

Preſſure 3 or 4 hundred atmoſpheres.

Similar experiments on bituminous matter.

I have likewiſe made ſome experiments with coal treated in the ſame manner as the carbonate of lime, but I have found it much leſs tractable, for the bitumen, when heat is applied to it, tends to eſcape by its ſimple elaſticity, whereas the carbonic acid in marble is in part retained by the chemical force of quick lime. I ſucceeded, however in conſtraining the bitumenous matter of the coal to a certain degree in red heats, ſo as to bring the ſubſtance into a complete fuſion, and to retain its faculty of burning with flame. But I could not accompliſh this in heats capable of agglutinating the carbonate; for I have found, where I rammed them ſucceſſively into the ſame tube, and where the veſſel has withſtood the expanſive force, that the carbonate has been agglutinated into a good lime-ſtone, but that the coal has loſt about half its weight, together with its power of giving flame when burnt, remaining in a very compact ſtate with a ſhining fracture. Although this experiment has not afforded the deſired reſult, it anſwers another purpoſe admirably well. It is known that where a bed of coal is croſſed by a dyke of Whinſtone, the coal is found in a peculiar ſtate in the immediate neighbourhood of the Whin, the ſubſtance in ſuch places being incapable of giving flame, it is diſtinguiſhed by the name of blind coal. Dr. Hutton has explained this fact by ſuppoſing that the bituminous matter of the coal has been driven by the local heat of the Whin into places of leſs intenſity, where it would probably be retained by diſtillation. Yet the whole muſt have been carried on under the action of a preſſure capable of conſtraining the carbonic acid of the calcareous ſpar which occurs frequently in ſuch rocks. In the laſt mentioned experiment, we have a perfect repreſentation of the natural fact ſince the coal has loſt its petroleum, whilſt the chalk in contact with it has retained its carbonic acid.

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* Philoſ. Journal, quarto ſeries, I. 459.

Remarkable fact of a production reſembling blind coal.

I have made ſome experiments of the ſame kind with vegetable and animal ſubſtances. I found their volatility much greater than that of coal, and I was compelled with them to work in heats below redneſs; for even in the loweſt red heat they were apt to deſtroy the apparatus. The animal ſubſtance I commonly uſed was horn, and the vegetable ſaw-duſt of fir. The horn was incomparably the moſt fuſible and volatile of the two. In a very ſlight heat it was converted into a yellow red ſubſtance like oil, which penetrated the clay tubes through and through. In theſe experiments I therefore made uſe of tubes of glaſs. It was only after a conſiderable portion of the ſubſtance had been ſeparated from the maſs that the remainder aſſumed the clear black peculiar to coal. In this way I obtained coal, both from ſaw-duſt and from horn, which yielded a bright flame in burning.

Animal and vegetable matters thus treated.

The mixture of the two produced a ſubſtance having exactly the ſmell of ſoot or coal tar. I am therefore ſtrongly inclined to believe that animal ſubſtance, as well as vegetable, has contributed towards the formation of our bituminous ſtrata. This ſeems to confirm an opinion advanced by Mr. Keir, which has been mentioned to me ſince I made this experiment. I conceive

Probability that coal is of animal as well as vegetable origin.

ceive that the coal which now remains in the world is but a ſmall portion of the organic matter originally depoſited, the moſt volatile parts having been driven off by the action of heat before the temperature had riſen high enough to bring the ſurrounding ſubſtance into fuſion, ſo as to confine the elaſtic fluids and ſubject them to compreſſion.

Horn totally volatilized under ſtrong but not extreme preſſure.

In ſeveral of theſe experiments, I found that when the preſſure was not great, when equal, for inſtance, only to 80 atmoſpheres, that the horn employed was diſſipated entirely, the glaſs tube which had contained it being left almoſt clean, yet undoubtedly if expoſed to heat without compreſſion, and protected from the contact of the atmoſphere, the horn would leave a cinder or coak behind it, of matter wholly devoid of volatility. Here then it would ſeem as if the moderate preſſure, by keeping the elements of the ſubſtance together, had promoted the general volatility, without being ſtrong enough to reſiſt that expanſive force, and thus, that the whole had eſcaped. This reſult, which I ſhould certainly not have foreſeen in theory, may perhaps account for the abſence of coal in ſituations where its preſence might be expected on principles of general analogy.

I have ſhewn ſeveral ſpecimens of theſe reſults to my friends, in particular to Lord Webb Seymour, Mr. Playfair, and Mr. Davy, who have agreed in thinking that the inveſtigation is now brought to ſuch a ſtage of advancement, that the reſult ought to be made public.

I propoſe in the courſe of next winter to lay before the Society a particular account of all theſe reſults, and of the methods followed in obtaining them. In the mean time I ſhall now ſubmit a few of them to the inſpection of the gentlemen preſent.

Exhibition and deſcription of the reſults of carbonate expoſed to heat under ſtrong preſſure.

Nos. 1, 2, 3, 4, 5, 6 and 7, were all produced in ſeparate experiments from pounded carbonate of lime. No. 1, was amongſt the firſt of my ſucceſsful reſults, having been obtained in 1799. It is a firm ſtone, requiring a ſmart blow of a hammer to break it. It was incloſed in a cartridge of paper, the mark of which it ſtill bears. The other ſix are ſtill harder and more compact, approaching nearly in theſe qualities to common lime-ſtone. Nos. 2, 4, and 7, poſſeſs a degree of ſemi-tranſparency moſt remarkable in No. 4, and all of theſe ſpecimens exhibit an uneven fracture, approaching to that of bees-wax and marble. Their colours are variouſly though ſlightly tinged with yellow and blue; in particular No. 3, which though produced from common white chalk, reſembles a yellow marble. Nos. 3, 5 and 6 have taken a tolerable poliſh. No. 7 contains a ſhell introduced along with the pounded chalk, and now cloſely incorporated with it.

Nos. 8, 9, 10, 11, all formed from pieces of chalk expoſed unbroken to heat and preſſure. No. 8 is remarkable for a ſhining grain and ſemi-tranſparency. Nos. 9 and 10 ſhew parallel planes like internal ſtratification which has often appeared in chalk, in conſequence of the action of heat, though nothing of the kind could be ſeen in the native maſs. No. 11, very compact, and of a yellow colour.

Great increaſe of denſity and cloſeneſs.

By various trials, to be given in detail hereafter, it appears that the carbonate in all theſe experiments has undergone a great diminution of bulk, amounting in ſome caſes to more than $\frac{1}{3}$ of the original maſs; and that its denſity has been proportionably increaſed. At the ſame time the poroſity of the ſubſtance has diminiſhed in a ſtill higher degree. Thus it is found that chalk in its natural ſtate abſorbs and retains from 20 to 25 per cent. of water; but after being expoſed to heat under compreſſion, that it does not abſorb quite 0.2 per cent. or the 500 part of its weight.

Other ſpecimens of bodies expoſed to heat under ſtrong preſſure.

Nos. 12, 13. Examples of welding, in which the pounded chalk has been incorporated with a lump of chalk, upon which it had been rammed, ſo that their joining is hardly viſible in the fracture.

Nos. 14, 15, 16. Shewing the fuſion of the carbonate well advanced, with a conſiderable action on the porcelain tube. In No. 15, the rod of chalk is half melted, and a yellow ſubſtance produced by a mixture of the carbonate with the porcelain. No. 16 is a lump of chalk, in a ſtate indicating ſoftneſs; a piece of porcelain, which lay in contact with it, having ſunk a little into the ſubſtance of the carbonate.

Nos. 17 and 18, being delicate, are incloſed in tubes of glaſs. No. 17, formed from pounded chalk, ſhews in one part the moſt complete formation of ſpar with its rhomboidal fracture I have ever obtained. The carbonate having loſt ſome of its carbonic acid, had crumbled ſo much in its eſſential parts by the action of the air, that the cryſtallization was no longer viſible, and I had given up the ſpecimen for loſt till within

theſe

Other ſpecimens of bodies expoſed to heat under ſtrong preſſure.

theſe few days. When employed in examining theſe reſults, a maſs of the carbonate broke in two, and exhibited the fracture now before us nearly in as good a ſtate as it was originally. I immediately incloſed it in a glaſs tube, and ſealed it up with wax, ſo that I have hopes of preſerving it. In the mean time I am happy to ſhew it entire to the Society. No. 18, likewiſe from pounded chalk is perfectly freſh and entire, though made more than a year ago; it ſhews ſome beautiful clear cryſtals of ſpar in parallel plates, but is ſo ſmall as to require the uſe of a glaſs.

Nos. 19, 20, 21, ſhew examples of fuſion and action on the tubes. In number 19, a ſhell is finely united to ſome pounded chalk. In No. 20, the maſs originally of pounded chalk is ſinking upon itſelf, and acting at the ſame time upon the tube. The pure carbonate in its fracture ſhewing brilliant facettes of cryſtallization. In No. 21, the carbonate in a ſtate like the laſt; the compound of porcelain and carbonate ſhewing its liquidity by penetrating the tube ſo as to form a diſtinct vein, and then ſpreading on its outſide to a conſiderable extent, terminating with the black line above alluded to.

Nos. 22, 23, 24, give proofs of entire fuſion. In No. 22, we ſee two porcelain tubes incloſed for preſervation in a glaſs tube, the ſealed end of which muſt be held downwards, to ſhew the poſition in which the experiment was made. The innermoſt porcelain tube ſtands with its muzzle upwards, and the outermoſt covers it in the inverſe poſition; the carbonate was contained in the inner tube. During the action of heat, the barrel failed ſuddenly, and the carbonate has boiled over the lips of the inner tube, running down, as here appears, almoſt to its bottom; thus proving that immediately previous to the failure of the apparatus, the carbonate had been in a liquid ſtate. No. 23, two maſſes of carbonate, welded together in a complete ſtate of froth. The ſubſtance ſhining and tranſparent. No. 24, two ſeparate maſſes expoſed together to heat; one from pounded chalk, now in a ſtate quite like the laſt; the other put in as a lump of chalk dreſſed flat at both ends, and a letter cut on each end (as done in many of the experiments.) It is in a ſhining and almoſt tranſparent ſtate; at one end the flat form and the letter are ſtill viſible; the other end is completely rounded in fuſion, with a gloſſy ſurface.

Nos. 25, 26, reſults of coal. No. 25, produced by the fuſion of common coal under preſſure in low red heat. It gave flame powerfully. No. 26, coal produced from horn. It is a ſhining black ſubſtance, exactly reſembling pitch or petroleum, and burns with a bright flame.

JAMES HALL.

IX.

Atmoſpherical Air not a mechanical Mixture of the Oxigenous and Azotic Gaſes, demonſtrated from the Specific Gravities of theſe Fluids. In a Letter from Mr. John Gough.

To Mr. NICHOLSON.

SIR,

I HAVE already attempted to prove common air not to be a mechanical mixture. The arguments which I uſed for the purpoſe, were drawn from the properties of refracted light and the motion of ſounds through elaſtic fluids. Such proofs are of an indirect nature; and though the mathematician may ſee the force of them, they may not carry an equal degree of conviction to the chemiſt, who ought alſo to be convinced. The following obſervations apply more immediately to the ſubject; and I do not perceive how they can be refuted, unleſs this be done by diſputing the accuracy of the experiments on which they are founded. The preſent enquiry has the recommendation of being ſtatical, and the data of my calculations are borrowed from certain experiments made by Mr. Kirwan and M. Lavoiſier. According to the former gentleman, if the weight * of atmoſpherical air be denoted by 1000, that of an equal bulk of carbonic acid gas will be 1500, of oxigenous gas 1103, of azotic gas 985. An attempt to diſcover the comparative weights of a number of gaſes is a difficult undertaking; but the preceding ratios may be uſed with ſome degree of confidence, becauſe they have been eſtabliſhed by a philoſopher of the higheſt reputation. Then in order to apply them to the buſineſs in hand, we will ſuppoſe the weight of 100 cubic inches of common air to be denoted by unity; though the real weight of the quantity may be ſtated at 31 grains,

Introductory remarks.

Experimental data.

Equations to find the oxigen in air.

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* Eſſay on Phlogiſton.

grains, according to Mr. Kirwan. Alſo let the characters w, x, and y, expreſs reſpectively the cubic inches of the carbonic acid, the oxigenous, and azotic gaſes, which compoſe the mixture contained in 100 ſolid inches. Now $w + x + y = 100$ inches by hypotheſis. But if 100 inches of air be denoted by unity, the ſame bulk of carbonic acid gas will be expreſſed by the fraction $\frac{1500}{1000}$, by the experimental data; conſequently the weight of this gas in the mixture will be found by the following proportion; *i. e.* as $100 : \frac{1500}{1000} :: w : \frac{1500w}{100000}$. In like manner, the weight of the oxigen is found to be $\frac{1103x}{100000}$, and that of the azote is $\frac{985y}{100000}$. Now the ſum of theſe weights is equal to unity by the premiſes of the calculation: if then the equation be multiplied by 100000 on the common denominator of the unknown quantities, it aſſumes the following form; *i. e.* $1500w + 1103x + 985y = 100000$. Thus it appears that we have only two equations, when the quantities to be determined are three; this circumſtance leaves the problem unlimited; that is, the value of one of the quantities muſt be diſcovered without the aid of calculation.

Air contains carbonic acid.

M. Chaptal takes no notice of the carbonic acid in his obſervations on the conſtitution of common air; which appears to be an overſight, at leaſt in a general view of the ſubject; for various proceſſes of nature as well as art conſtantly diſcharge this gaſeous acid into the atmoſphere, where its preſence is alſo indicated by quick-lime being converted into a carbonate, when placed in open ſituations. Theſe facts amount to more than a probability, that the lower parts of the atmoſphere contain a ſlight admixture of the carbonic acid: this ſmall portion, however, was undoubtedly retained, in a great meaſure, by the azotic gas which Mr. Kirwan uſed in his experiments; becauſe he prepared it from common air, which was confined over mercury, together with a paſte of ſulphur and the filings of iron.

w and y determined.

This ſingle conſideration induces me to follow the example of M. Chaptal in making w of no value; and the ſtep may be taken with the greateſt ſecurity in the preſent inſtance, becauſe it increaſes the value of x, if it alter it at all; that is, the amount of the oxigenous gas, as found by calculation, will exceed the truth, on the ſuppoſition that the carbonic acid remained in the azotic gas, which Mr. Kirwan weighed. If, then, w be put equal to nothing, the preceding equations aſſume the following forms; *i. e.* $x + y = 100$, and $1103\,x + 985\,y = 100000$. Multiply the former by 985, ſubtract the product from the latter, and you will have the following equation; *i. e.* $118\,x = 1500$: Hence by diviſion, $x = 12,\frac{42}{59}$; and the exceſs of 100 above this number gives $y = 87,\frac{17}{59}$. The ſteps of this proceſs have been detailed minutely, with a view to enable the chemical reader, who has the leaſt knowledge of algebra, to conſider the grounds of the following concluſions, and to form his own judgment reſpecting the weight of them.

Air not a mixture of oxigenous and azotic gaſes.

It is evident from the foregoing calculation, that if 100 parts of a mixture of the oxigenous and azotic gaſes, having the ſpecific gravity of common air, were deprived of their oxigen in a graduated tube, the reſiduum would meaſure ſomething more than 87 ſuch parts, *i. e.* $87,\frac{17}{59}$. But when an equal bulk of the atmoſpherical fluid is treated in the ſame manner, the portion of it which remains unabſorbed, is much leſs than the preceding quantity; M. Chaptal makes it to be 72 ſuch parts, and ſome writers call it 78, the mean of which is 75. Now if we ſubtract any one of theſe numbers from the reſiduum fixed by calculation, a difference will be found, which cannot be referred to the unavoidable imperfections of eudiometers, becauſe it could hardly eſcape obſervation in any inſtrument of the kind, the leaſt exceſs being more than nine parts, or nearly a tenth, of the whole ſcale of 100 parts. On the other hand, a mixture of 72 parts azote and 28 oxigen, or of 78 of the former and 22 of the latter, exceeds an equal bulk of common air in weight; conſequently the atmoſpherical fluid is not a mechanical mixture of the two gaſes in queſtion, if any credit be due to the experimental data.

Air a gaſeous oxide of azote.

Though air has been ſhewn not to conſiſt of the oxigenous and azotic gaſes ſimply mixed together, it is certainly a compound that may be reſolved into theſe two principles: For beſides ſupporting reſpiration and combuſtion, it converts metals into oxides, and the nitrous gas into nitric acid; therefore it contains the oxigenating principle. On the other hand, when air is employed to oxigenate bodies, the reſiduum of it is azotic gas of greater or leſs purity; conſequently the atmoſpherical fluid is a gaſeous oxide of azote, which can be decompoſed by art, though chemiſts have not as yet diſcovered a certain method of producing it at pleaſure, by uniting the oxigenating matter to the azotic baſe.

Facts

Statical analyſis of air.

Facts have obliged me to give a name to common air, which has been hitherto excluſively applied to the dephlogiſticated nitrous air of Dr. Prieſtly. This remark being made for the ſake of perſpicuity, I will endeavour, in the next place, to give a ſtatical analyſis of the atmoſpherical fluid. If 100 cubic inches of common air weigh 31 grains, the weight of an equal bulk of azotic gas will be 30.535 grains; becauſe as 1000 : 31 : : 985 : 30.535 : in like manner the weight of 100 cubic inches of oxigenous gas will be found to be 34.193 grains. The quantity of azotic gas in 100 cubic inches of air, will be ſtated at 75 inches in the preſent calculation, for the following reaſons: *Firſt*, Becauſe 75 is the mean of 72 and 78; *ſecond*, Becauſe M. Lavoiſier found, that four inches of oxigenous gas and 16 inches of air ſaturate equal quantities of the nitrous gas. Theſe premiſes being ſettled, we ſhall find the weight of 75 inches of azotic gas to be 22.90125 grains; conſequently the weight of the oxigen gas in 100 cubic inches of common air, is the exceſs of 31 above the laſt number, or 8.09875 grains; therefore as 34.193 : 100 inches : : 8.09875 grains : 23.685 inches, which is the meaſure of the oxigen gas in 100 cubic inches of common air, when the azotic gas is ſtated at 75 inches. Thus it appears, that if 100 parts of the atmoſpherical fluid were decompoſed, the elementary gaſes would occupy together no more than 98.685 ſuch parts; and a difference of a like nature will be obſerved, if the azotic gas be called 72 or 78 *per cent.*; hence it follows that the denſity of air is leſs than that of the mechanical mixture of its elements. This poſition may appear paradoxical at the firſt view, but chemiſtry can furniſh various inſtances of compounds, which are ſpecifically lighter than the aggregates of their ingredients.

The oxigen of the air compared with the oxigenous gas.

This analyſis muſt remain incomplete, until the powers of the oxigen of the atmoſphere have been compared with the correſponding effect of an equal weight of the oxigenous gas. Such an attempt, however, is liable to great uncertainty; becauſe the experiments which ſhould ſupply the neceſſary data, are variouſly repreſented by different writers. M. Lavoiſier ſays, that four parts of oxigenous gas and nearly 16 of common air, oxigenate equal bulks of nitrous gas, namely 17,$\frac{1}{3}$ parts. On the contrary, M. Chaptal found by repeated experiments, that 12, or at moſt 13, parts of air were ſufficient to ſaturate the ſame quantity of nitrous gas. The data of the following calculation are taken partly from the one and partly from the other author: I have ſuppoſed with M. Lavoiſier, that three inches of the oxigenous will acidify 13 inches of the nitrous gas: M. Chaptal is followed in other reſpects, namely, the atmoſphere is imagined to contain 72 *per cent.* of azote, and 39 inches of air are made equivalent to 52 of nitrous gas. The calculation, which is formed upon theſe ſuppoſitions, will in all probability prove incorrect; but I have ventured to inſert it, as being a novelty, which is likely to excite enquiry. If 39 inches of air can ſaturate 52 of nitrous gas, 100 inches of the former require 133.333 of the latter. Again, if 13 inches of nitrous gas demand three of oxigen, 133.333 of the former will require 30.303 of the latter. But the weight of 30.303 inches of oxigen gas amounts to 10.361 grains, which is equivalent in effect to 100 inches of air. Now 72 inches of azotic gas weigh 21.985 grains, which being taken from 31 grains, leaves 9.015 grains for the weight of the oxigenous part of 100 inches of air; in round numbers, 90 grains of the latter kind of oxigen are equal in effect to 103 of the former.

I know that M. Lavoiſier, in ſpeaking of the conſtitution of nitric acid, makes 100 grains of it contain 64 grains of the nitrous and 36 of the oxigenous gas; or 173 inches of the former and 105 of the latter. Had the data of the preceding calculation been taken from this ratio, the comparative ſuperiority of atmoſpherical oxigen would have been much greater than it appears to be by the laſt paragraph. A preference, however, has been given to the preceding hypotheſis, becauſe I deſire to excite enquiry, rather than expect to eſtabliſh any thing of a permanent nature concerning the ſubject.

The origin of the queries in the Phil. Jour. vol. viii. p. 246.

The preceding arguments, in conjunction with others of a kind more uncertain than themſelves, ſuggeſted to me the probability of atmoſpherical oxigen poſſeſſing a greater degree of efficacy than an equal weight of vital air. This idea occurred to me ſeveral months ago; and I at length reſolved to propoſe it to the public in a number of hypothetical queſtions, which appeared in your Journal for Auguſt. Theſe queries are evidently borrowed from ſome ingenious ſpeculations, that were publiſhed in the eighth volume of the ſame work *; for the queries imply, in common with the remarks here alluded to, that the oxigenating matter is water chemically united to the poſitive power of the galvanic pile. In order to accommodate this notion to the caſe of common air, I have ſuppoſed that the oxigen of the atmoſphere receives a ſtronger charge of the power in queſtion, than that which is imparted to vital air; in conſequence of which it takes up an additional quantity of water, upon being diſunited from the azotic baſis, and is thus converted into oxigenous gas. Should future experiments diſcover that bodies, which are oxigenated by given portions of common air in contact with water, acquire more weight than the air loſes, the diſcovery will undoubtedly open a wide field for enquiry.

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* Page 88.

JOHN GOUGH.

Middleſhaw, Sept. 5, 1804.

X.

Letter from THOMAS YOUNG, *M. D. F. R. S. &c. announcing the Diſcovery of a new moving Star, by Mr.* HARDING, *of Lilienthal; and on other Subjects.*

To Mr. NICHOLSON.

DEAR SIR,

New planet lately diſcovered.

I HAVE juſt received a letter from Dr. Gauſs of Brunſwick, F. R. S. dated September 11, in which he informs me that, a few days before, Mr. Harding at Lilienthal had diſcovered "a new moving ſtar, moſt probably another new planet of our ſolar ſyſtem." Dr. Gauſs is certainly a perſon on whoſe judgment much dependance may be placed: He has ſent ſome further particulars of the diſcovery to the Aſtronomer Royal.

Maximum of denſity of water.

I take this opportunity of making a criticiſm on a work of great experimental merit which has lately appeared. It relates to the apparent expanſion of water in a veſſel of glaſs. Mr. Leſlie calculates, from the ſuppoſition that the expanſion of water is proportional to the ſquare of the degrees of temperature above the freezing point, and from the expanſion of glaſs meaſured by General Roy, that the apparent maximum of denſity ſhould be at 6° of the decimal ſcale: but he has taken in this calculation the mean rate of expanſion from the freezing to the boiling point, inſtead of the expanſion at the boiling point, which, upon the ſuppoſition, is twice as great; hence the apparent maximum, on the ſame grounds of calculation, ſhould be at 3° of the decimal ſcale, or at 37.4° of Fahrenheit. Mr. Dalton attempts to avoid this difficulty by ſuppoſing that the thin bulb of a thermometer expands more than the glaſs tube employed by General Roy.

Elaſticity of bodies.

The ſame ingenious author has made experiments on the elaſticity of different ſubſtances, by meaſuring the depreſſion of the middle of a bar ſupported at its extremities; but if I am not much miſtaken, his inferences from them are by no means accurate. Mr. Leſlie gives 671625 feet for the height of a column of deal equivalent to its elaſticity; the true height reſulting from his experiments, taking into conſideration alſo the inequality of curvature, appears to me to be 4664000 feet; which is ſtill little more than half as much as would be inferred from the experiments of Chladni on the longitudinal ſounds of fir wood. There muſt have been ſome inaccuracy in Mr. Leſlie's experiments on ſteel differently tempered: for it appears from the direct experiments of Coulomb on the flexure of bars, as well as from thoſe of Chladni, and ſome of my own, upon ſound, that the ultimate elaſticity of ſteel in ſmall tenſions is the ſame, whether it is harder or ſofter. This may appear at firſt ſight paradoxical, but it admits of a ſufficient explanation, which, together with many other illuſtrations of various parts of natural philoſophy, will probably before long be laid before the public.

Your very obedient ſervant,

THOMAS YOUNG.

Welbeck Street, Sep. 22, 1804.

XI.

Deſcription of an Inſtrument to aſcertain the Number of Lifts made from a Mine, in any given Time. By Mr. JOHN ANTIS [*].

SIR,

Inſtrument for counting the lifts from a mine.

I HAVE lately been encouraged by ſome gentlemen engaged in ſome coal mines in this neighbourhood, to invent a machine, which would infallibly tell the number of boxes of coals drawn out of a pit in the courſe of a week, or any given time. I have juſt now completed a model, which I think would anſwer the purpoſe extremely well.

Before, however, I make it known, I thought I would inquire of you, whether ſuch an invention might be of a more extenſive uſe, and as ſuch would deſerve the attention of the Society.

The machine is very ſimple, and need not be expenſive; its properties are as follow:

1. It is of no conſequence if the pit be ten, twenty, forty, or more yards deep. It will, notwithſtanding this difference, only point out one box at once; and, ſuppoſing the pit to be ſeveral hundred yards deep, the principle will remain the ſame, and the machine could be eaſily adapted for it.

2. It is likewiſe of no conſequence if the coals be drawn up by a hand windlaſs, or any other machine turned by horſes or other powers.

3. No account is neceſſary to be kept for a whole week, or any given time, as the machine can be calculated for any quantity of coals whatever, that can be drawn up in a given time. Only allowance muſt be made for the number of perſons that go down and up in the ſame way.

I have not yet heard of any contrivance of this kind; and therefore, if there be any ſuch, it is unknown to me. Whoever knows the fraud, which is but too often committed by bankſmen, in accounting for the quantity of coals procured from a pit, will readily admit the utility of ſuch a contrivance. And it may moſt likely alſo be uſeful in tin, lead, and other mines. Should the Society be of the ſame opinion, then I will ſend you a ſtill more complete model than at preſent. Your ſpeedy anſwer will very much oblige

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[*] From the Tranſactions of the Society of Arts, 1803.

Inſtrument for counting the lifts from a mine.

Your moſt obedient Servant,

JOHN ANTIS.

Fulneck, *March* 6, 1802.

Charles Taylor, Eſq.

SIR,

I HEREWITH ſend you a model of my machine, which I have before mentioned to you, for pointing out any number of boxes or baſkets of coals, or other minerals, which are drawn out of a pit in any given time. I ſhall be much obliged to you, if you will lay it before the Society for the Encouragement of Arts, &c. for their inſpection, and ſhall be glad that they find it of public utility.

It will eaſily be perceived, that by enlarging the wheels and multiplying the cogs of them, the number of yards required for the depth of a mine, as likewiſe the number of boxes which are wanted to be pointed out may be carried to any extent.

I am ſo little acquainted with coal-pits, or any other mines, that I do not know the technical terms by which the different parts are called, nor all the methods now uſed to draw up thoſe materials; I muſt therefore beg the indulgence of the Society, if I do not always expreſs myſelf with accuracy.

By inventing this machine, I aimed at aſcertaining the number of boxes, my ideas reſpecting which I will endeavour to deſcribe, to the beſt of my abilities.

Firſt, the rope muſt be ſo long, that it can be faſtened at its middle to the windlaſs, and that each end thereof may reach to the bottom of the pit. This precaution will not be neceſſary, if it can be prevented by any other means from ſhifting or ſliding, which I leave to others, more acquainted than I am with the preſent practice, to effect.

The roller A, *Plate* VII. *Fig.* 1, in the model, with the endleſs ſcrew, repreſents the gudgeon in full ſize of a hand windlaſs, ſuch as are uſed in this neighbourhood, particularly where the pits are of conſiderable depth. As no great ſtreſs is laid upon this ſcrew, it may be made, in order to ſave labour, of braſs,

and

Inſtrument for counting the lifts from a mine.

and driven, or otherwiſe faſtened, upon any gudgeon. I ſuppoſe the windlaſs to be one yard in circumference; and conſequently forty revolutions of the windlaſs are required before the firſt wheel B, upon which the braſs catch C is fixed, turns once round. The pit, therefore, may be only ten yards, or from ſeventy to ſeventy-five yards deep, yet it will make no difference. All that is required to regulate the machine to the depth of the pit, is as follows:—If the pit be little more or leſs than ten yards, the firſt wheel muſt be ſo placed in the endleſs ſcrew, that the braſs catch C may be at the beginning, but juſt eſcaped from the cogs of the ſecond wheel D. It will then only want five or ſix revolutions of the windlaſs backwards, before the ſaid catch is ready to operate again. But ſuppoſing the pit to be ſeventy yards deep, in this caſe, the catch C muſt be placed ſo as nearly to be ready to act. By turning backwards, the wheel may very nearly make two revolutions; and as it can be ſtill turned as far the other way, the catch will only act once, provided the ſaid wheel does not quite make two revolutions.

The model is about the ſize required for a common hand windlaſs. The index will at once ſhow how many times fifty boxes, and how many above and below that number, have been drawn up: not one of them can eſcape. Allowance, however, muſt be made for the number of perſons who go up and down in the ſame way. Beſides this, ſince one box is always coming up, whilſt the other is going down, the numbers pointed at muſt be doubled, except a perſon was deſirous to have a machine acting both ways, which would hardly be worth while. As the machine is at preſent, though it points out a thouſand boxes only, yet it will ſerve for two thouſand.

The ſame mechaniſm, with very little variation, may be fixed to a horſe machine, called a gin, in this neighbourhood; viz. as a gin may be ten yards or more in circumference, the action would be too ſlow, if moved by an endleſs ſcrew; a pinion, with four leaves, will be found much more convenient. This pinion is fixed to the end of the upper gudgeon, which enters the box containing the machinery. As ſome accuracy is required to make ſuch a pinion work well, and theſe machines are often coarſely conſtructed, it will be neceſſary only to make about one inch and a half of the end of the ſaid gudgeon exactly round, namely, that part of it which goes through the bottom of the box, and then faſten the latter ſo, that it can give way to any remaining inaccuracy of the other parts of the gudgeon.

Inſtrument for counting the lifts from a mine.

I have ſent a drawing * of ſuch a machine, which will be eaſily underſtood, ſince the principle is the ſame as in the model. But as gins are commonly made uſe of in deep mines, I have made it upon a larger ſcale; viz. the firſt wheel has ſixty-four cogs; and this, with a pinion of four leaves, will require ſixteen revolutions of the gin, for one of the wheel. Allowing ten yards for the circumference of the gin, it will make one hundred and ſixty yards of rope; and conſidering that the ſaid wheel can nearly make two revolutions, and that the catch will ſtill act only once, it will conſequently ſerve for a pit of three hundred yards deep, as well as for one of ten or twenty.

In this machine, the three wheels are placed one on another, and the index is divided; viz. the fifty on the ſecond, and the 1000 on the third wheel. As the wheels move, the two hands are ſtanding, which will point out the numbers as exactly as thoſe in the model. Indeed, machines for hand windlaſſes may be made in the very ſame way, and thereby be more ſimple, though the difference is but ſmall; viz. the box would require but one lid and lock, and the two ſmaller wheels become unneceſſary.

Though in ſetting the machine ſo as to begin counting with one, it may eaſily be effected by lifting out the ſpring, and then turning the wheel with the other hand, till both hands ſtand upon one, yet, in the double index, the upper hand muſt only be ſet thus with the wheel, and the lower may be moved round on its ſocket.

In my humble opinion, my machines may be uſeful in two ways; firſt, to the honeſt banksman, as they will ſave him the trouble of noting down each box which is drawn up, and prevent miſtakes; ſecondly, in the prevention of fraud, which will be their greateſt and moſt important uſe. To obtain this end, it is neceſſary that the machine be ſo contrived, that nothing eſſentially wanted can be diſengaged. I have therefore made the model ſo as to fruſtrate any ſuch deſign. All is within the box; and neither the endleſs ſcrew can be diſengaged

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* This drawing is in the Society's poſſeſſion.

Inſtrument for counting the lifts from a mine.

gaged from the wheel, nor the ſpring; nor can the number on the index be altered without firſt opening the box. This latter can be made of iron, if found neceſſary, with a good lock to it; otherwiſe, the ſtreſs is ſo very little, that even one of wood, made in all reſpects like the model, would anſwer, if it could be ſecured from the effects of the weather. In the machine likewiſe, repreſented by the drawing, nothing can be altered, except the box be firſt opened. This machine is faſtened to the beam in which the upper gudgeon turns. To effect this, the bottom of the box muſt have two projecting ends with holes in them, larger than neceſſary to receive a ſtrong pin or rivet; which pins muſt have large heads, ſo that when properly driven or riveted in, the box may be able to give way to any remaining inaccuracy in the lower parts of the gudgeon, as I before obſerved. Thoſe machines for hand windlaſſes, like the model, are fixed to the ſide of the poſt into which one of the gudgeons turns; but perhaps theſe might likewiſe be contrived ſo, that the gudgeon with the endleſs ſcrew may be a piece by itſelf, attached only to the windlaſs by a ſquare, or otherwiſe, as is often practiſed in other machineries, ſuch as cotton-mills, &c.

I am, Sir,

Your obedient Servant,

JOHN ANTIS.

Fulneck, May 18, 1802.

Charles Taylor, Eſq.

Reference to the Model ſent by Mr. ANTIS *to the Society.*

Plate VII. *Fig.* 1. A. The roller, with an endleſs ſcrew, to be the ſize of the gudgeon of a hand windlaſs.

B. The firſt wheel, of forty teeth, with a braſs catch C. It may be moved backwards or forwards by the endleſs ſcrew.

D. A ratchet wheel with fifty teeth, which, when the wheel B is put in motion one way, the catch will ſlide over the teeth of it; but, on turning it the other way, the catch will drop into and forward this wheel one tooth, in which ſituation it is prevented from returning by the ſpring E, placed on the ſide of the box which holds a tooth on the ſide of the wheel.

F. A ſmall pinion of four teeth, on the back part of the axle of the wheel D. This pinion moves a large wheel on the other ſide of the box.

Fig.

Inſtrument for counting the lifts from a mine.

Fig. 2. G. The large wheel laſt mentioned: it has eighty teeth.

H. A ſmaller wheel of fifty teeth, fixed faſt on the face of the wheel G; which wheel H works a wheel I, of the ſame number of teeth, ſhown by dotted lines behind the figured dial-plate.

K. The ſhort hand, or finger of the dial, is placed upon an arbor or collar of this wheel I, and moves with it.

The long finger, or hand of the dial-plate, goes upon a ſquare on the end of the ſpindle on which the above arbor moves; which ſpindle moves forward this long finger one diviſion in fifty of the outer circle marked on the dial-plate, every time the rope, or any thing attached to it, has been down to the bottom of the mine, and returned to the top.

The inner circle of the face of the dial is marked in diviſions of fifty each. When therefore fifty draughts up and down have been made from the mine, the ſhorter hand will be found, if both hands were originally ſet correctly at the top, to have moved to the firſt interior diviſion marked 50; and ſo on, in proportion, will advance as more draughts are made.

K, Is a board placed under the wheels B D, in *Fig.* 1, and which ſeparates them from the other wheel-work in *Fig.* 2, where only a few teeth of the wheel B appear behind it.

L L, Show the temporary handles in both figures; and the poſition of thoſe letters denote that the gudgeon of the windlaſs, when the machine is in actual uſe, ſhould be there joined to, or make a part of the roller A.

Fig. 3, Shows, on an enlarged ſcale, the form of the catch C. The ſteel ſpring M preſſing againſt the pin N, returns the catch to the tooth of the wheel D, when it has been forced back; and a pin fixed underneath the catch moves in a groove O, made in the wheel B, to prevent the catch being preſſed too far back, or thrown out improperly by the ſpring.

A door is fixed on each ſide the box, and ſhould be locked, to prevent the hands being altered unknowingly.

The drawing of a machine for ſimilar purpoſes, intended to be uſed with a gin or horſe-wheel, alluded to in Mr. Antis's letter, is in the Society's poſſeſſion, if a reference thereto be thought neceſſary.

On

XII.

On Galvaniſm. In a Letter from RA. THICKNESSE, *Eſq.*

To Mr. NICHOLSON.

SIR, *Wigan, Sept.* 20, 1804.

On the cauſes which retard diſcovery.

ALTHOUGH the production of the electric fluid by the galvanic pile has never yet been ſatisfactorily accounted for, it appears to me ſomewhat eaſy to be explained, from a conſideration of the principles of chemiſtry. This, I am aware, is a very bold opinion from a man who is a mere dabbler in philoſophy; but as it muſt be allowed that the ableſt philoſophical inquirers and experimentaliſts have been guilty of extraordinary overſights, I truſt my preſumption is excuſable. When an ardent mind has once entered a wrong path in purſuit of knowledge, it is too intent upon the objects before it to turn aſide, and too anxious to proceed to be induced to look back. Thus chemiſts fifty years ago thought they obtained earth from water, becauſe they omitted to weigh the veſſels in which they made their experiments; and thus, perhaps, the profeſſors of galvaniſm at the preſent day, being of opinion, as I believe they all are, that the electric fluid proceeds from the metals, devote little of their attention to the fluid employed in the pile, conſidering it almoſt as a mere conductor. Nothing can be more obvious to a chemiſt now than the neceſſity of weighing the matter he ſubjects to experiment, and alſo the product; yet the diſcovery alone of this neceſſity overturned the opinions, and falſified the wiſdom, of all the previous cultivators of the ſcience, and led to the modern theory of chemiſtry and its improvements; an omiſſion how trifling in itſelf to keep men (philoſophers too) poking in the dark for ages!

Argument or inference that water is compoſed of oxigen, hidrogen, and electricity.

Water, we are told, is compoſed of certain parts of oxigen and hidrogen; but, as to form theſe bodies, from a ſtate of gas at leaſt, into water, it is neceſſary to paſs an electric ſpark through, or rather *into* them (no matter whether the proceſs of formation be combuſtion or not), and as water owes its fluidity to heat, the matter of heat and the electric fluid being probably the ſame, or modifications of the ſame body, I think it reaſonable to aſſume that water is compoſed of oxigen, hydrogen, and *the electric fluid.*

The

Decompoſition of fluids in galvaniſm aſcribed, not to the action of electricity, but to chemical affinity.

The experiments by which M. Volta and others have endeavoured to ſhew that piles compoſed of metals only afford the galvanic fluid, are ſo little demonſtrative of it, that they ſeem to me equally illuſtrative of the contrary; and I believe that all galvanic piles muſt, with the intervention of fluids, be compoſed of two metals, or *other ſubſtances*, which have an affinity, the one for oxigen and the other for hidrogen; for water and other fluids made uſe of in galvanic piles, are decompoſed; and not, I conceive, as hath generally been imagined, by the electricity of the metals; but by the action of affinities. For inſtance, in a pile formed with zinc, copper, and water, the oxigen and hidrogen of the water having a ſtronger affinity for the zinc and copper (the oxigen for the zinc and the hidrogen for the copper) than for each other, unite with them, the water being conſequently decompoſed, and the electric or galvanic fluid, which was contained in it, conſequently ſet at liberty.

Fuller explanation from the facts.

In ſupport of this theory I may alledge as facts, that the firſt ſhock from a galvanic pile is generally the ſtrongeſt, when the energy of the affinities is the greateſt; that for a repetition ſome interval of time is requiſite, whilſt the decompoſition is taking place; that the metals in the pile act, not according to their quantities, but according to their ſurfaces—the greater theſe are, the greater being the quantity of water acted upon; that the zinc is always oxidated, and the copper (or ſilver) always acted upon by the hidrogen, being rendered more brittle, &c.; that any alteration in the arrangement of the pile, which brings two pieces of the ſame metal to the ſame ſtratum of water, inſtead of one piece of each metal (one to act on the oxigen and the other on the hidrogen), interrupts the proceſs, and, no decompoſition taking place, there is no electric fluid produced; and it may alſo be added, that this laſt is a fact which cannot otherwiſe be accounted for.

Objection, that the galvanic ſtream decompoſes water, anſwered.

I am not ignorant that a ſtream of the galvanic fluid from the pile, paſſed through water, decompoſes it; but it is no proof, I preſume, that water contains none of this fluid, becauſe it is decompoſed by a greater quantity than will chemically mix with it. If we pour a ſmall quantity of water impregnated with carbonic acid gas into lime-water, the lime is precipitated, or *compoſed*, the lime-water becoming turbid;

but if we add a further quantity, the lime is again decompoſed, and the lime-water becomes again perfectly clear.

I am, Sir,

Your moſt obedient ſervant,

RA. THICKNESSE.

XIII.

Deſcription of an Inſtrument for drawing in true Perſpective from Nature, and of another of conſiderable Simplicity and Cheapneſs for delineating Ovals. In a Letter from a Correſpondent, R. B.

To Mr. NICHOLSON.

SIR,

Introduction.

AS I obſerve that you are willing, in your capacity of Journaliſt, to lay before the public any ſketch or outline of invention that may promiſe to be uſeful, whether in its ultimate ſtate of improvement or not, I am encouraged now and then to ſend my thoughts, queries, obſervations, or news, as they may occur. The following inſtruments are offered to your notice, in hopes they may appear in your excellent collection.

Inſtrument for drawing in perſpective.

Ramsden

[illegible]

Improve

suggests

Patented

[illegible]

Fig. 1, *Plate* VIII. is a ſketch of an inſtrument for perſpective, made ſome years ago ~~by Dolland~~, and of which I know not the inventor. A teleſcope or camera is ſuſpended vertically on a frame by an univerſal joint or jimbals. Horizontal rays A, are directed down the tube by a plane mirror B, and are again rendered horizontal, and turned to the eye through a ſide hole in the tube, by another mirror C. At the lower end is a pencil E ſliding in a well-fitted ſocket, and preſſed gently downwards by a weight or ſpring; or ſtill better, by the hand only. The reſult or uſe is, that while the images are in ſucceſſion brought into apparent contact with a point in the field of view, the pencil may be employed in tracing them in true perſpective upon the table beneath *.

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* There is an omiſſion of the grey or rough glaſs, if the drawing be meant for a camera; or of the eye-piece, if it be a teleſcope. The firſt focal convergence muſt be made in theſe, and not at the eye.—N.

Fig.

Fig. 2 repreſents a ſimple rule and ſtring for drawing ovals on paper. A C B is a ſilken thread, fixed at A, and capable of being lengthened, ſhortened, and fixed by a ſcrew B at the other end. This ſcrew B can be placed, by a longitudinal groove in the ruler, at any diſtance from A, and can be made to pinch the thread upon any one of the diviſions of the rule. At C is a pencil to be moved in the bend of the thread. It muſt be held upright, and it would be eaſy to contrive means of keeping it ſo; but it does not ſeem an object of ſufficient neceſſity to add to the price of the inſtrument.

Inſtrument for drawing ovals.

In the uſe, ſet A at one focus of the intended oval and B at the other. Allow the ſtring to extend till the pencil marks the extremity of the conjugate diameter. Draw the ſemi-oval by moving the pencil along in the ſtretched thread: Then reverſe the points A and B, placing them reſpectively on the foci occupied before by each other. Draw the other ſemi-oval, which completes the figure.

I am, Sir,
Your obliged correſpondent,
R. B.

XIV.

On the Computation of Tables of Squares and Cubes. In a Letter from H. G.

To Mr. NICHOLSON.

SIR, *Sept.* 8, 1804.

As I can employ my time in a more uſeful, pleaſant, and advantageous way than by making tables of any kind, I have no deſire on my own account to trouble your readers with the following remarks, and ſhall therefore leave it entirely to your choice to notice them or not in your Journal.

Introduction.

I am, Sir,
Your humble ſervant,
H. G.

YOUR correſpondent E. O. in his note, p. 79 of your Journal for this month, has very properly pointed out an error in

On the computation of tables of ſquares and cubes.

On the computation of tables of ſquares and cubes.

in the rule laid down by me in p. 150 of your Journal for July laſt, which will be corrected by ſubſtituting the word *given* for the word *next*.

But he does not ſeem to be aware that the rule I propoſed for finding the ſquares of roots in an arithmetical progreſſion of thoſe roots, is preciſely the ſame as that he has uſed and elucidated in pages 5 and 6 of your Journal for this month.

Neither has he made me a convert to his doctrine, that addition ſhould be uſed in preference to ſubtraction; for the one appears to me as eaſy an operation as the other, for all the purpoſes to which it is applied in the conſtruction of the tables in queſtion.

With reſpect to the conſtruction of the table of cubes, I will barely ſtate my method of computing the cubes of the ſame roots E. O. has calculated, in pages 8 and 9 of your Journal for this month, and will leave it to thoſe who may be inclinable to enter on the inveſtigation, to determine which method will be moſt advantageouſly practiſed, his or mine, only obſerving, that E. O. has a continual repetition of the firſt differences, which I have entirely avoided without adding any figures, in other reſpects, to thoſe he has uſed.

I ſtill think my method of computing the table of cubes is not more liable to inaccuracy than that E. O. has adopted; but in order to avoid almoſt the poſſibility of error in my computation, and at the ſame time to add to, examine, and correct Mr. Councer's table, I would place the cubes of half the given roots × 8. at ſuch diſtances under each other, as would enable me to interpolate my calculations of the cubes of twice thoſe roots, as it is done in the left hand part of the following elucidation; and then it is evident, every other cube obtained by the calculation muſt be the ſame as that obtained from the table; and thus a proof of the accuracy of Mr. Councer's table, and of the continuation of it, would go hand in hand.

TABLE.

TABLE,

H. G.'s Method to obtain the Cubes of certain Roots. Cubes.	Roots.	*Supposed Mr. Councer's.* Roots:	Cubes:	Cubes of H. G.'s Roots *per contra*, or Cubes of twice the Roots opposite.
$a = 26560$ =	18736316416000 = A - - - - - -	13280^3 =	2342039552000 × 8 =	18736316416000
$b = 26559$ =	18734200194879 = B			
	2116221121 = C = A − B			
	159360 = D = $6a$			
$c = 26561$ =	18738432796481 = E = D + C + A			
	2116380481 = F = E − A			
	159366 = G =			
$d = 26562$ =	18740549336328 = H = G + F + E .	13281^3 =	2342568667041 × 8 =	18740549336328
	2116539847 = I = H − E			
	159372 = K = G + 6 = $6d$			
$e = 26563$ =	18742666035547 = L = K + I + H			
	2116699219 = M = L − H			
	159378 = N = K + 6 = $6e$			
$f = 26564$ =	18744782894144 = O = N + M + L .	13282^3 =	2343097861768 × 8 =	18744782894144
	2116858597 = P = O − L			
	159384 = Q = N + 6 = $6f$			
$g = 26565$ =	18746899912125 = R = Q + P + O			

XV.

Letter to the Editor from Mr. WILLIAM HENRY, *in reply to Mr. Gough.*

Mancheſter, September 13, 1804.

SIR,

Previous remarks.

NOTHING was farther from my intention, when I communicated to you the "Illuſtrations of Mr. Dalton's Theory of Mixed Gaſes," than to enter into controverſy reſpecting a doctrine, to the defence of which I may naturally be ſuppoſed to be much leſs competent than its author. Yet it is certainly required of me, both by the reſpectful attention due to your correſpondent Mr. Gough, and to others of your readers, either to explain and ſupport, or to relinquiſh, if erroneous, the opinions reſpecting which I have publicly expreſſed a coincidence with Mr. Dalton. The laſt alternative I do not, at preſent, feel diſpoſed to adopt, becauſe I am far from being convinced by Mr. G.'s reaſoning; and in the explanation, which I am about to offer, I ſhall confine myſelf to thoſe proofs of the new theory of mixed gaſes, which are furniſhed by my own experiments; leaving to Mr. Dalton the more important office of eſtabliſhing its fundamental evidences.

The quantity of gas abſorbed being as the preſſure, the laſt is taken to be the cauſe,

It is by no means clear to me, whether or not Mr. Gough denies the principle, "that the relation between gaſes and water is altogether a mechanical one."* To me this appears as legitimate an inference, as can poſſibly be deduced in phyſics; for the quantity of every gas, abſorbed by water, follows exactly the ratio of the preſſure: And, ſince it is a rule in philoſophizing, that effects of the ſame kind, though differing in degree, are produced by the ſame cauſe, it is perfectly ſafe to conclude, that every, even the minuteſt portion of any gas, in a ſtate of abſorption by water, is retained entirely by incumbent preſſure. There is no occaſion, therefore, to call in the aid of the law of chemical affinity, when a mechanical law fully and ſatisfactorily explains the appearances. And when the effect ceaſes, it is equally conformable to juſt reaſoning to infer, that this happens, in every caſe, ſolely in conſequence of the relaxation or removal of that mechanical power, which held the gas in its ſituation. Under all circumſtances, therefore, when a gas eſcapes from water, whether by placing the aqueous mixture under an exhauſted receiver, or in an atmoſphere of a different gas, the cauſe operating its eſcape, muſt be one and identical, viz. the diminution of mechanical preſſure. Before we account for any effect by compariſon of affinities, the affinities themſelves ſhould be proved to exiſt. But with reſpect to the relation of gaſes to water, the proof fails in every inſtance; for how can that effect be fairly aſcribed to chemical affinity which is deſtroyed, as is the connection of every gas with water, by an unmixed mechanical cauſe; and is it not abſurd to compare powers which have no exiſtence in nature?

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* Certain acid gaſes, the muriatic for inſtance, are obviouſly excluded.

—and the preſſure is of no other gas than that abſorbed.

Admitting then the connection between gaſes and water to be entirely dependent on phyſical preſſure, there naturally ariſes out of this law an explanation of the curious fact, which I have aſcertained, that each gas, when abſorbed by water, is retained in its place by an atmoſphere of no other gas but of its own kind. Under any other atmoſphere, the abſorbed gas eſcapes, even without agitation, though this certainly accelerates the event. Now the ſubaqueous gas can only accompliſh its change of place by virtue of ſome active principle or power inherent in it, and cauſing its movement; and this power is its elaſticity, which is not counteracted by that of an incumbent gas of a different ſort. The fact affords, therefore, ſomething further than "probability," that the particles of gaſes preſs only on thoſe of their own kind; for to ſay that the elaſticity of the ſubaqueous gas is not counteracted by any incumbent one of a different ſort, is to aſſert in other terms, that the one is not preſſed by the other,—the principle which I am ſolicitous to eſtabliſh.

The theory is an induction from facts; capable of mathematical treatment, but founded on obſervation.

The above remarks are the only ones which I deem it within my province to urge in reply to Mr. Gough. Before cloſing this letter, however, I muſt exculpate myſelf from the charge of attempting to uphold a mechanical theory, by probabilities drawn from chemical facts. This ſtatement is not correct; the facts which I have alledged are purely ſtatical, and my object has been to prove that they were before erroneouſly included under the laws of chemical affinity, with which, in reality they have no connection. Theſe mechanical phenomena, I have brought in ſupport of the general principle "that the particles of gaſes preſs only on thoſe of their own kind;" and in ſo applying them, I truſt I have conformed to the rigorous laws of philoſophical induction. Such general principles are not uncommon in natural ſcience; and though, like the law of gravitation and ſome other leſs comprehenſive ones, they may be mathematically purſued and inveſtigated, yet they are not derived from mathematical reaſoning, but from a method of enquiry which Newton himſelf did not diſdain to employ,—that of induction on the baſis of experiment and obſervation. The ſpeculations of Mr. Dalton, being founded on well aſcertained facts, appear therefore to me to be fairly entitled to the appellation of *theory*, and not to be included within the definition of *hypotheſis*,* handed down to us from the father of experimental philoſophy. I remain, very truly, Sir,

Your's, &c.

WILLIAM HENRY.

XVI.

Deſcription of a very ſimple and cheap Contrivance for making Port Folios of large Dimenſions. By the late JAMES MALTON, *Eſq.*†

SIR,

Method of conſtructing large port-folios.

AS I well know the great inconvenience experienced by artiſts and collectors of prints and drawings, from the want of portfolios of dimenſions capable of incloſing large ſubjects, and as I alſo well know that the means uſed by the Society for the Encouragement of Arts, &c. to promulgate knowledge and uſeful information are earneſt as they are extenſive, I am induced to lay before that body a port-folio of my conſtruction, which I perſuade myſelf poſſeſſes every advantage that can be wiſhed.

The difficulty, or rather the impoſſibility of obtaining caſes or port-folios, as large as are ſometimes requiſite, has given riſe to many expenſive contrivances, to the ſame end; or large prints,

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* "Quicquid ex phaenomenis non deducitur, *hypotheſis* vocanda eſt." Princ. LIII. in Bruckeri Hiſt. Crit. Phil. Tom. IV. p. 646.

† Society of Arts, 1803.

Method of conſtructing large port-folios.

prints, &c. muſt be kept in rolls, to their almoſt certain deſtruction, by frequency of rolling; or at leaſt they are thus expoſed to the danger of being cruſhed by accident. Milled paſteboards, of which port-folios are made, are not manufactured above a certain moderate ſize: to exceed that ſize in a port-folio, is an undertaking of no inconſiderable trouble, in paſting, glueing, and preſſing them together. On inquiring of Mr. Newman, of Soho-ſquare, (a manufacturer of theſe articles) how he managed to make port-folios above the ordinary dimenſions, he informed me, it was an undertaking of trouble, and related his having made one for a gentleman, by attaching ſixteen of the largeſt milled boards together; that the materials alone coſt five guineas; and that its weight was greater than one man could lift.

My method of conſtruction obviates all diſadvantages—weight, expence, and trouble; and port folios of any dimenſions may very readily be manufactured by the ſimple application of two ſtraining-frames, covered on both ſides with canvas, and papered; and connected, as all port-folios are, by leather at the back, or with wooden backs, the ſides being connected by hinges. Thus a port-folio may be made capable of holding the largeſt cartoons, maps, and prints; and poſſeſſing another great advantage, beſides that of not bellying or ſwagging, when laid againſt a wall, as thoſe conſtructed of paſteboard do, to their own deſtruction, and material injury of the things they contain.

A frame of four feet by three will be ſtrong enough, if made of deal. The ſtiles are four inches wide by half an inch thick; and they have a middle upright ſtile of the ſame width, with angle pieces at the corners, as is ſhown in the engraving, *Fig.* 1. A frame of much greater dimenſions may require two middle upright ſtiles; and, if very large, a middle longitudinal ſtile, as is ſhown in the engraving, *Fig.* 2.

If the frames are made of mahogany, they need not exceed 3-16ths of an inch in thickneſs; but, of whatever wood they are made, it muſt be well ſeaſoned, or they will warp. A padlock may be applied to ſuch caſes, for the protection of their contents.

On this conſtruction I have made two port-folios, one of which I have had in uſe theſe ten years. A handſome one, of tolerably large dimenſions, I have ſent with this paper, for the

Method of conſtructing large port-folios.

the inſpection of the Society. The outer ſtiles of it are of mahogany, which, beaded, forms the out edge in a neat manner. Its ſimple formation, its lightneſs, and its firm flatneſs, muſt be obvious to every one; and I am of opinion the Society will obtain the thanks of all collectors and artiſts (if they think it worthy of inſertion in the Volume of their Tranſactions) by making this ſimple matter publicly known. For my own part, I ſhall be highly gratified in having contributed to the comfort of artiſts and collectors, in preſerving their valuable reſearches.

I am, Sir,
Your obedient Servant,
JAMES MALTON.

Norton-ſtreet, June 25, 1802.

Reference to the Engraving, Plate VIII.

Fig. 4. One of the ſides of the frame for a port-folio, the dimenſions four feet by three: it may be made of deal or fir wood. The ſtiles, four inches wide by half an inch thick; the middle upright ſtile to be of the ſame width. It ſhould have angle pieces within the corners, to keep them firm.

Fig. 3. Shows a ſide of another frame, where much larger dimenſions are required; it ſhould then have two upright ſtiles, and a middle longitudinal ſtile, all within the frame, and angle pieces at the corners. If it is made of mahogany, inſtead of deal, the ſtiles may be reduced nearly one-fourth in breadth and thickneſs; the wood, in either caſe, ſhould be well ſeaſoned, that it may not be liable to warp.

XVII.

Experiments and Calculations relative to Phyſical Optics, By THOMAS YOUNG, *M. D. F. R. S. From the Philoſophical Tranſactions for* 1804.

(Concluded from p. 64.)

Exp. 2. So likewiſe the creſted fringes of Grimaldi.

Exper. 2. THE creſted fringes deſcribed by the ingenious and accurate GRIMALDI, afford an elegant variation of the preceding experiment, and an intereſting example of a calculation grounded on it. When a ſhadow is formed by an object which has a rectangular termination, beſides the uſual external fringes, there are two or three alternations of colours, ginning from the line which biſects the angle, diſpoſed on ch ſide of it, in curves, which are convex towards the bi-cting line, and which converge in ſome degree towards it, as y become more remote from the angular point. Theſe nges are alſo the joint effect of the light which is inflected rectly towards the ſhadow, from each of the two outlines of e object. For, if a ſcreen be placed within a few inches of e object, ſo as to receive only one of the edges of the ſhadow, e whole of the fringes diſappear. If, on the contrary, the ctangular point of the ſcreen be oppoſed to the point of the adow, ſo as barely to receive the angle of the ſhadow on s extremity, the fringes will remain undiſturbed.

II. COMPARISON OF MEASURES, DEDUCED FROM VARIOUS EXPERIMENTS.

Compariſon of meaſures.

If we now proceed to examine the dimenſions of the fringes, under different circumſtances, we may calculate the differences of the lengths of the paths deſcribed by the portions of light, which have thus been proved to be concerned in producing thoſe fringes; and we ſhall find, that where the lengths are equal, the light always remains white; but that, where either the brighteſt light, or the light of any given colour, diſappears and reappears, a firſt, a ſecond, or a third time, the differences of the lengths of the paths of the two portions are in arithmetical progreſſion, as nearly as we can expect experiments of this kind to agree with each other. I ſhall compare, in this point of view, the meaſures deduced from ſeveral experiments of Newton, and from ſome of my own.

In the eighth and ninth obſervations of the third book of Newton's Optics, ſome experiments are related, which, together with the third obſervation, will furniſh us with the data neceſſary for the calculation. Two knives were placed, with their edges meeting at a very acute angle, in a beam of the ſun's light, admitted through a ſmall aperture; and the point of concourſe of the two firſt dark lines bordering the ſhadows of the reſpective knives, was obſerved at various diſtances. The reſults of ſix obſervations are expreſſed in the firſt three lines of the firſt Table. On the ſuppoſition that the dark line is produced by the firſt interference of the light reflected from the edges of the knives, with the light paſſing in a ſtraight line between them, Compariſon of meaſures.

we may aſſign, by calculating the difference of the two paths, the interval for the firſt diſappearance of the brighteſt light, as it is expreſſed in the fourth line. The ſecond Table contains the reſults of a ſimilar calculation, from Newton's obſervations on the ſhadow of a hair; and the third, from ſome experiments of my own, of the ſame nature: the ſecond bright line being ſuppoſed to correſpond to a double interval, the ſecond dark line to a triple interval, and the ſucceeding lines to depend on a continuation of the progreſſion: The unit of all the Tables is an inch.

TABLE I. *Obſ.* 9. N.

Diſtance of the knives from the aperture						101.
Diſtances of the paper from the knives	$1\frac{1}{2}$,	$3\frac{1}{3}$,	$8\frac{3}{5}$,	32,	96,	131.
Diſtances between the edges of the knives, oppoſite to the point of concourſe	.012,	.020,	.034,	.057,	.081,	087.
Interval of diſappearance	.0000122,	.0000155,	.0000182,	.0000167,	.0000166,	.0000166.

TABLE II. *Obſ.* 3. N.

Breadth of the hair		$\frac{1}{280}$.
Diſtance of the hair from the aperture		144.
Diſtances of the ſcale from the aperture	150,	252.
(Breadths of the ſhadow	$\frac{1}{54}$,	$\frac{1}{9}$.)
Breadth between the ſecond pair of bright lines	$\frac{2}{47}$,	$\frac{4}{17}$.
Interval of diſappearance, or half the difference of the paths	.0000151,	.0000173.
Breadth between the third pair of bright lines	$\frac{4}{73}$	$\frac{3}{10}$.
Interval of diſappearance, $\frac{1}{4}$ of the difference	.0000130,	.0000143.

TABLE III. *Exper.* 3.

Breadth of the object	.434.
Diſtance of the object from the aperture	125.
Diſtance of the wall from the aperture	250.
Diſtance of the ſecond pair of dark lines from each other	1.167.
Interval of diſappearance, $\frac{1}{3}$ of the difference	.0000149.

Exper. 4.

Breadth of the wire	.083.
Diſtance of the wire from the aperture	32.
Diſtance of the wall from the aperture	250.
(Breadth of the ſhadow by three meaſurements	.815, .826, or .827; mean, .823.)
Diſtance of the firſt pair of dark lines	1.165, 1.170, or 1.160; mean, 1.165.
Interval of diſappearance	.0000194.
Diſtance of the ſecond pair of dark lines	1.402, 1.395, or 1.400; mean, 1.399.
Interval of diſappearance	.0000137.
Diſtance of the third pair of dark lines	1.594, 1.580, or 1.585; mean, 1.586.
Interval of diſappearance	.0000128.

It appears, from five of the ſix obſervations of the firſt Table, in which the diſtance of the ſhadow was varied from about 3 inches to 11 feet, and the breadth of the fringes was increaſed in the ratio of 7 to 1, that the difference of the routes conſtituting the interval of diſappearance, varied but one-eleventh at moſt; and that, in three out of the five, it agreed with the mean, either exactly, or within $\frac{1}{160}$ part. Hence we are warranted in inferring, that the interval appropriate to the extinction of the brighteſt light, is either accurately or very nearly conſtant.

But it may be inferred, from a compariſon of all the other obſervations, that when the obliquity of the reflection is very great, ſome circumſtance takes place, which cauſes the interval thus calculated to be ſomewhat greater: thus, in the eleventh line of the third Table, it comes out one-ſixth greater than the mean of the five already mentioned. On the other hand, the mean of two of Newton's experiments and one of mine, is a reſult about one-fourth leſs than the former. With reſpect to the nature of this circumſtance, I cannot at preſent form a decided opinion; but I conjecture that it is a deviation of ſome of the light concerned, from the rectilinear direction aſſigned to it, ariſing either from its natural diffraction, by which the magnitude of the ſhadow is alſo enlarged, or from ſome other unknown cauſe. If we imagined the ſhadow of the wire, and the

Compariſon of meaſures.

fringes neareſt it, to be ſo contracted that the motion of the light bounding the ſhadow might be rectilinear, we ſhould thus make a ſufficient compenſation for this deviation; but it is difficult to point out what preciſe tract of the light would cauſe it to require this correction.

The mean of the three experiments which appear to have been leaſt affected by this unknown deviation, gives .0000127 for the interval appropriate to the diſappearance of the brighteſt light; and it may be inferred, that if they had been wholly exempted from its effects, the meaſure would have been ſomewhat ſmaller. Now the analogous interval, deduced from the experiments of Newton on thin plates, is .0000112, which is about one-eighth leſs than the former reſult; and this appears to be a coincidence fully ſufficient to authoriſe us to attribute theſe two claſſes of phenomena to the ſame cauſe. It is very eaſily ſhown, with reſpect to the colours of thin plates, that each kind of light diſappears and reappears, where the differences of the routes of two of its portions are in arithmetical progreſſion; and we have ſeen, that the ſame law may be in general inferred from the phenomena of diffracted light, even independently of the analogy.

The diſtribution of the colours is alſo ſo ſimilar in both caſes, as to point immediately to a ſimilarity in the cauſes. In the thirteenth obſervation of the ſecond part of the firſt book, Newton relates, that the interval of the glaſſes where the rings appeared in red light, was to the interval where they appeared in violet light, as 14 to 9; and, in the eleventh obſervation of the third book, that the diſtances between the fringes, under the ſame circumſtances, were the 22d and 27th of an inch. Hence, deducting the breadth of the hair, and taking the ſquares, in order to find the relation of the difference of the routes, we have the proportion of 14 to $9\frac{1}{4}$, which ſcarcely differs from the proportion obſerved in the colours of the thin plate.

We may readily determine, from this general principle, the form of the creſted fringes of Grimaldi, already deſcribed; for it will appear that, under the circumſtances of the experiment related, the points in which the differences of the lengths of the paths deſcribed by the two portions of light are equal to a conſtant quantity, and in which, therefore, the ſame kinds of light ought to appear or diſappear, are always found in equilateral hyperbolas, of which the axes coincide with the outlines of the ſhadow, and the aſymptotes nearly with the diagonal line. Such, therefore, muſt be the direction of the fringes; and this concluſion agrees perfectly with the obſervation. But it muſt be remarked, that the parts near the outlines of the ſhadow, are ſo much ſhaded off, as to render the character of the curve ſomewhat leſs decidedly marked where it approaches to its axis. Theſe fringes have a ſlight reſemblance to the hyperbolic fringes obſerved by Newton; but the analogy is only diſtant.

III. APPLICATION TO THE SUPERNUMERARY RAINBOWS.

Application of the doctrine of the interference of light to the ſupernumerary rainbows of Langwith and Daval.

The repetitions of colours ſometimes obſerved within the common rainbow, and deſcribed in the Philoſophical Tranſactions, by Dr. Langwith and Mr Daval, admit alſo a very eaſy and complete explanation from the ſame principles. Dr. Pemberton has attempted to point out an analogy between theſe colours and thoſe of thin plates; but the irregular reflection from the poſterior ſurface of the drop, to which alone he attributes the appearance, muſt be far too weak to produce any viſible effects. In order to underſtand the phenomenon, we have only to attend to the two portions of light which are exhibited in the common diagrams explanatory of the rainbow, regularly reflected from the poſterior ſurface of the drop, and croſſing each other in various directions, till, at the angle of the greateſt deviation, they coincide with each other, ſo as to produce, by the greater intenſity of this redoubled light, the common rainbow of 41 degrees. Other parts of theſe two portions will quit the drop in directions parallel to each other; and theſe would exhibit a continued diffuſion of fainter light, for 25° within the bright termination which forms the rainbow, but for the general law of interference, which, as in other ſimilar caſes, divides the light into concentric rings; the magnitude of theſe rings depending on that of the drop, according to the difference of time occupied in the paſſage of the two portions, which thus proceed in parallel directions to the ſpectator's eye, after having been differently refracted and reflected within the drop. This difference varies at firſt, nearly as the ſquare of the angular diſtance from the primitive rainbow: and, if the firſt additional red be at the diſtance of 2° from the red of the rainbow, ſo as to interfere a little with the primitive violet, the fourth additional red will be at a diſtance of nearly 2° more;

Application of the doctrine of the interference of light to the ſupernumerary rainbows of Langwith and Daval.

more; and the intermediate colours will occupy a ſpace nearly equal to the original rainbow. In order to produce this effect, the drops muſt be about $\frac{1}{76}$ of an inch, or .013, in diameter: it would be ſufficient if they were between $\frac{1}{70}$ and $\frac{1}{80}$. The reaſon that ſuch ſupernumerary colours are not often ſeen, muſt be, that it does not often happen that drops ſo nearly equal are found together: but, that this may ſometimes happen, is not in itſelf at all improbable; we meaſure even medicines by dropping them from a phial, and it may eaſily be conceived that the drops formed by natural operations may ſometimes be as uniform as any that can be produced by art. How accurately this theory coincides with the obſervation, may beſt be determined from Dr. Langwith's own words.

"Auguſt the 21ſt, 1722, about half an hour paſt five in the evening, weather temperate, wind at north-eaſt, the appearance was as follows: The colours of the primary rainbow were as uſual, only the purple very much inclining to red, and well defined: under this was an arch of green, the upper part of which inclined to a bright yellow, the lower to a more duſky green: under this were alternately two arches of reddiſh purple, and two of green: under all, a faint appearance of another arch of purple, which vaniſhed and returned ſeveral times ſo quick, that we could not readily fix our eyes upon it. Thus the order of the colours was, I. Red, orange-colour, yellow, green, light blue, deep blue, purple. II. Light green, dark green, purple. III. Green, purple. IV. Green, faint vaniſhing purple. You ſee we had here four orders of colours, and perhaps the beginning of a fifth; for I make no queſtion but that what I call the purple, is a mixture of the purple of each of the upper ſeries with the red of the next below it, and the green a mixture of the intermediate colours. I ſend you not this account barely upon the credit of my own eyes; for there was a clergyman and four other gentlemen in company, whom I deſired to view the colours attentively, who all agreed, that they appeared in the manner that I have now deſcribed. There are two things which well deſerve to be taken notice of, as they may perhaps direct us, in ſome meaſure, to the ſolution of this curious phenomenon. The firſt is, that the breadth of the firſt ſeries ſo far exceeded that of any of the reſt, that, as near as I could judge, it was equal to them all taken together. The ſecond is, that I have never obſerved theſe inner orders of colours in the lower parts of the rainbow, though they have often been incomparably more vivid than the upper parts, under which the colours have appeared. I have taken notice of this ſo very often, that I can hardly look upon it to be accidental; and, if it ſhould prove true in general, it will bring the diſquiſition into a narrow compaſs; for it will ſhow that this effect depends upon ſome property which the drops retain, whilſt they are in the upper part of the air, but loſe as they come lower, and are more mixed with one another." Phil. Tranſ. Vol. XXXII. p. 243.

From a conſideration of the nature of the ſecondary rainbow, of 54°, it may be inferred, that if any ſuch ſupernumerary colours, were ſeen attending this rainbow, they would neceſſarily be external to it, inſtead of internal. The circles ſometimes ſeen encompaſſing the obſerver's ſhadow in a miſt, are perhaps more nearly related to the common colours of thin plates as ſeen by reflection.

IV. ARGUMENTATIVE INFERENCE RESPECTING THE NATURE OF LIGHT.

Argumentative inferences.

The experiment of Grimaldi, on the creſted fringes within the ſhadow, together with ſeveral others of his obſervations, equally important, has been left unnoticed by Newton. Thoſe who are attached to the Newtonian theory of light, or to the hypotheſes of modern opticians, founded on views ſtill leſs enlarged, would do well to endeavour to imagine any thing like an explanation of theſe experiments, derived from their own doctrines; and, if they fail in the attempt, to refrain at leaſt from idle declamation againſt a ſyſtem which is founded on the accuracy of its application to all theſe facts, and to a thouſand others of a ſimilar nature.

Homogeneous light has oppoſite qualities at equal diſtances along its courſe, by which neutralization or extinction of the light may be effected.

From the experiments and calculations which have been premiſed, we may be allowed to infer, that homogeneous light, at certain equal diſtances in the direction of is motion, is poſſeſſed of oppoſite qualities, capable of neutraliſing or deſtroying each other, and of extinguiſhing the light, where they happen to be united; that theſe qualities ſucceed each other alternately in ſucceſſive concentric ſuperficies, at diſtances which are conſtant for the ſame light, paſſing through the ſame medium. From the agreement of the meaſures, and from the ſimilarity of the phenomena, we may conclude, that

Light moves more ſlowly in denſer mediums.

Light and ſound ſtrongly reſemble each other.

that theſe intervals are the ſame as are concerned in the production of the colours of thin plates; but theſe are ſhown, by the experiments of Newton, to be the ſmaller, the denſer the medium; and, ſince it may be preſumed that their number muſt neceſſarily remain unaltered in a given quantity of light, it follows of courſe, that light moves more ſlowly in a denſer, than in a rarer medium: and this being granted, it muſt be allowed, that refraction is not the effect of an attractive force directed to a denſer medium. The advocates for the projectile hypotheſis of light, muſt conſider which link in this chain of reaſoning they may judge to be the moſt feeble; for, hitherto, I have advanced in this Paper no general hypotheſis whatever. But, ſince we know that ſound diverges in concentric ſuperficies, and that muſical ſounds conſiſt of oppoſite qualities, capable of neutraliſing each other, and ſucceeding at certain equal intervals, which are different according to the difference of the note, we are fully authoriſed to conclude, that there muſt be ſome ſtrong reſemblance between the nature of ſound and that of light.

There is probably no inflecting medium.

I have not, in the courſe of theſe inveſtigations, found any reaſon to ſuppoſe the preſence of ſuch an inflecting medium in the neighbourhood of denſe ſubſtances as I was formerly inclined to attribute to them; and, upon conſidering the phenomena of the aberration of the ſtars, I am diſpoſed to believe, that the luminiferous ether pervades the ſubſtance of all material bodies with little or no reſiſtance, as freely perhaps as the wind paſſes through a grove of trees.

Practical application of the facts.

The obſervations on the effects of diffraction and interference, may perhaps ſometimes be applied to a practical purpoſe, in making us cautious in our concluſions reſpecting the appearances of minute bodies viewed in a microſcope. The ſhadow of a fibre, however opaque, placed in a pencil of light admitted through a ſmall aperture, is always ſomewhat leſs dark in the middle of its breadth than in the parts on each ſide. A ſimilar effect may alſo take place, in ſome degree, with reſpect to the image on the retina, and impreſs the ſenſe with an idea of a tranſparency which has no real exiſtence: and, if a ſmall portion of light be really tranſmitted through the ſubſtance, this may again be deſtroyed by its interference with the diffracted light, and produce an appearance of partial opacity, inſtead of uniform ſemitranſparency. Thus, a central dark ſpot, and a light ſpot ſurrounded by a darker circle, may reſpectively be produced in the images of a ſemitranſparent and an opaque corpuſcle; and impreſs us with an idea of a complication of ſtructure which does not exiſt. In order to detect the fallacy, we may make two or three fibres croſs each other, and view a number of globules contiguous to each other; or we may obtain a ſtill more effectual remedy by changing the magnifying power; and then, if the appearance remain conſtant in kind and in degree, we may be aſſured that it truly repreſents the nature of the ſubſtance to be examined. It is natural to inquire whether or no the figures of the globules of blood, delineated by Mr. Hewſon in the Phil. Tranſ. Vol. LXIII. for 1773, might not in ſome meaſure have been influenced by a deception of this kind: but, as far as I have hitherto been able to examine the globules, with a lens of one-fiftieth of an inch focus, I have found them nearly ſuch as Mr. Hewſon has deſcribed them.

REMARKS ON THE COLOURS OF NATURAL BODIES.

On the colours of natural bodies

Exper. 5. I have already adduced, in illuſtration of Newton's compariſon of the colours of natural bodies with thoſe of thin plates, Dr. Wollaſton's obſervations on the blue light of the lower part of a candle, which appears, when viewed through a priſm, to be divided into five portions. I have lately obſerved a ſimilar inſtance, ſtill more ſtrongly marked, in the light tranſmitted by the blue glaſs ſold by the opticians. This light is ſeparated by the priſm into ſeven diſtinct portions, nearly equal in magnitude, but ſomewhat broader, and leſs accurately defined, towards the violet end of the ſpectrum. The firſt two are red, the third is yellowiſh green, the fourth green, the fifth blue, the ſixth bluiſh violet, and the ſeventh violet. This diviſion agrees very nearly with that of the light reflected by a plate of air $\frac{1}{6840}$ of an inch in thickneſs, correſponding to the 11th ſeries of red and the 18th of violet. A ſimilar plate of metallic oxide, would perhaps be about $\frac{1}{15000}$ of an inch in thickneſs. But it muſt be confeſſed, that there are ſtrong reaſons for believing the colouring particles of natural bodies in general to be incomparably ſmaller than this; and it is probable that the analogy, ſuggeſted by Newton, is ſomewhat leſs cloſe than he be imagined. The light reflected by a plate of air, at any thickneſs nearly correſponding to the 11th red, appears to the eye to be very nearly white; but, under favourable circumſtances, the 11th red and the neighbouring colours may ſtill be diſtinguiſhed. The light of ſome kinds of coloured glaſs is pure red; that of others, red with a little green; ſome intercept all the light, except the extreme red and the blue. In the blue light of a candle, expanded by the priſm, the portions of each colour appear to be narrower, and the intervening dark ſpaces wider, than in the analogous ſpectrum derived from the light reflected from a thin plate. The light of burning alcohol appears to be green and violet only. The pink dye ſold in the ſhops, which is a preparation of the carthamus, affords a good ſpecimen of a yellow green light regularly reflected, and a crimſon probably produced by tranſmiſſion.

VI. EXPERIMENT ON THE DARK RAYS OF RITTER.

Dark rays of Ritter obſerved alſo by Dr. Wollaſton.

Exper. 6. The exiſtence of ſolar rays accompanying light, more refrangible than the violet rays, and cogniſable by their chemical effects, was firſt aſcertained by Mr. Ritter: but Dr. Wollaſton made the ſame experiments a very ſhort time afterwards, without having been informed of what had been done on the Continent. Theſe rays appear to extend beyond the violet rays of the priſmatic ſpectrum, through a ſpace nearly equal to that which is occupied by the violet. In order to complete the compariſon of their properties with thoſe of viſible light, I was deſirous of examining the effect of their reflection from a thin plate of air, capable of producing the well known rings of colours. For this purpoſe, I formed an image of the rings, by means of the ſolar microſcope, with the apparatus which I have deſcribed in the Journals of the Royal Inſtitution, and I threw this image on paper dipped in a ſolution of nitrate of ſilver, placed at the diſtance of about nine inches from the microſcope. In the courſe of an hour, portions of three dark rings were very diſtinctly viſible, much ſmaller than the brighteſt rings of the coloured image, and coinciding very nearly, in their dimenſions, with the rings of violet light that appeared upon the interpoſition of violet glaſs. I thought the dark rings were a little ſmaller than the violet rings, but the difference was not ſufficiently great to be accurately aſcertained; it might be as much as $\frac{1}{30}$ or $\frac{1}{40}$ of the diameters, but not greater. It is

They are reflected from a thin plate of air with the ſame modification of rings as viſible light is.

is the leſs ſurpriſing that the difference ſhould be ſo ſmall, as the dimenſions of the coloured rings do not by any means vary at the violet end of the ſpectrum, ſo rapidly as at the red end. For performing this experiment with very great accuracy, a helioſtate would be neceſſary, ſince the motion of the ſun cauſes a ſlight change in the place of the image; and leather, impregnated with the muriate of ſilver, would indicate the effect with greater delicacy. The experiment, however, in its preſent ſtate, is ſufficient to complete the analogy of the inviſible with the viſible rays, and to ſhow that they are equally liable to the general law which is the principal ſubject of this Paper. If we had thermometers ſufficiently delicate, it is probable that we might acquire, by ſimilar means, information ſtill more intereſting, with reſpect to the rays of inviſible heat diſcovered by Dr. Herſchel; but at preſent there is great reaſon to doubt of the practicability of ſuch an experiment.

SCIENTIFIC NEWS, ACCOUNT OF BOOKS, &c.

National Inſtitute.

National Inſtitute prizes.

THE claſs of mathematical and phyſical ſciences of the French National Inſtitute had a public ſitting on the 6th Meſſidor (June 25.) The ſubjects appointed for prizes were the following:

Planet Pallas.

Mathematics. "It is required to give a theory of the perturbations of the planet Pallas, diſcovered by M. Olbers."

The prize will be a gold medal, weighing one kilogramme, (35½oz. avoirdupois.)

Carbon in veg tables.

Natural Philoſophy. The claſs had propoſed for the ſubject of a prize the following queſtion. "To determine by experiment the different ſources of carbon in vegetables." The concurrence is prorogued until the 1ſt Germinal in the year 13 (March 22, 1805.)

Torpid ſtate of dormice, &c.

The concurrence for the prize upon the following queſtion is alſo prolonged to the ſame period. "To determine by anatomical and chemical obſervations and experiments, what are the phenomena of the torpid ſtate to which certain animals, ſuch as marmots, dormice, &c. are ſubject during the winter, with reſpect to the circulation of the blood, reſpiration, and irritability; to inveſtigate the cauſes of this ſleep, and why it is peculiar to theſe animals."

The value of theſe two prizes is doubled, and conſiſts in two kilogrammes of gold about 6800 francs each (£289.)

The claſs had propoſed for the ſecond time, the 15th Germinal of the year 10, as the ſubject of a prize to be decreed in the public ſitting of Meſſidor, the following queſtion.

erments. "What are the characters which diſtinguiſh, in vegetable and animal matters, thoſe bodies which ſerve as ferments from thoſe in which they produce the ſtate of fermentation."

The memoirs received not having anſwered the conditions of the program, and the claſs conſidering that this queſtion has been four years before the public, the ſubject is now withdrawn.

ſtronomy. rize gained by iazzi. Aſtronomy. Mr. Piazzi of Palermo has obtained the prize founded by M. De Lalande, in favour of the perſon who ſhall have made the moſt intereſting obſervation, or written the moſt uſeful memoir to the progreſs of aſtronomy. Mr. Piazzi is the diſcoverer of the planet Ceres.

eadings. The readings delivered during the ſitting were as follow:

1. Notice of the labours of the claſs during the year; the mathematical part by M. Delambre. 2. Notice of their labours; the phyſical part by M. Cuvier. 3. Account of a phyſico-mathematical theory of currents of water, by M. Prony. 4. A note reſpecting the amelioration of ſheep in the ſouth of France, by M. Teſſier. 5. General reflections on the productions of the vegetable kingdom in the Pyrenean mountains by M. Raymond. 6. Reflections on heat, by Count Rumford. 7. Extract of a memoir tending to illuſtrate the œconomical hiſtory of vegetables, cultivated or naturally growing in the Canaries, by M. Brouſſonet.

Extract of a Letter from PROFESSOR BODE, *Aſtronomer Royal, to Mr. A. F. Thoelden.*

Dated Berlin, Sept. 18.

Place of new planets. ON the firſt of September Mr. Harding diſcovered, at Lilienthal, a new moveable ſtar; it appears to be of the eighth magnitude; its motion retrograde towards the South. Probably this may be another new planet in the orbit of Ceres and Pallas.

1ſt Sept.	10^h.—Even.	RA.	2°	24′		Decl.	0°	37′		—N.
5 —	11 8′	-	1	51	51″	-	0	11	26″	S.
8 —	8 11	-	1	29	28	-	0	47	19	-
10 —	8 15	-	1	12	55	-	1	11	55	-

The two laſt obſervations were made by Dr. Olbers, at Bremen. I received this intelligence yeſterday, but at preſent this ſmall planet (there are now three) is not diſcoverable, on account of the moon-ſhine.

Extract of a Letter from Mr. G. B. GREENOUGH.

Correction of a detail in the account of Melograni's blow-pipe.

* * * * There is an overſight in the haſty account I ſent you of the Abbé Melograni's blow-pipe. No proviſion is made for admitting the external air. This may be done by ſeveral means. It may be admitted through that part of the axis which is oppoſite the nozel; but as this would require ſome contrivance which would render it defective in ſimplicity, I ſhould prefer the addition of a ſmall valve opening inwards at that part of each veſſel which is immediately oppoſite the neck that joins them. Whether this method was uſed in the actual inſtrument I cannot, at this diſtance of time, recollect.

Organic Remains of a former World. An Examination of the Mineralized Remains of the Vegetables and Animals of the Antediluvian World, generally termed extraneous Foſſils. By JAMES PARKINSON, *Hoxton. Vol.* I. *containing the Vegetable Kingdom. 4to. pp.* 471. *with* 10 *Plates* (£2 2 0) *Robſon, &c.*

Parkinſon's organic remains.

AS it would be impracticable in the general announcement of a work of the magnitude and importance of the preſent to give the reader any well filled outline of the plan, I muſt confine myſelf to ſtate in few words, that the deſcription and elucidation of organic foſſil bodies, though in many points of view of the higheſt intereſt, has not yet conſtituted the ſubject of a general treatiſe; notwithſtanding the various conſiderations reſpecting the local, the individual, and the chemical hiſtory of theſe wonderful bodies have occupied the reſearches of many able philoſophers in detached memoirs; and that the compoſition of ſuch an hiſtory demands a great exertion of labour, in conſulting and digeſting what has been

done

Parkinſon's organic remains.

done by others, an accurate, geological, and chemical view of the facts upon which ſcientific concluſions or deductions may be grounded, and above all, an ardent curioſity, with adequate leiſure and means to become acquainted with the ſpecimens themſelves:—And, after this preface, I muſt add, that fortunately for the advancement of this branch of knowledge, the author of the preſent work has amply vindicated his claim to theſe requiſites.

The work is written in the form of letters, and enlivened by an adoption of the ſtyle and ſome of the incidents of a traveller. Much of uſeful ſcience and clear explanations of the ſubject are to be found in the pages of this volume. The abridgements from other authors are full, and the quotations always given with the volume and page, which I mention more readily, as a ſlovenly habit of looſe quotation, or not quoting at all, is but too common, even with our writers of merit. Nine plates of the ſubjects are given, which do the higheſt credit to the deſigner and engraver, Springſguth, for their delicacy and beauty, and conſiderable pains have been taken in colouring them. The frontiſpiece, repreſenting the Ark upon Mount Ararat, deſigned by Corbould, is happy and appropriate; though the artiſt has taken a pictorial licence to repreſent the ſun and the rainbow nearly on the ſame azimuth, for which neither opticians nor correct obſervers will be diſpoſed to excuſe him.

The volume yet to be publiſhed will contain the animal kingdom; a ſubject perhaps ſtill more attractive than the vegetable. In the remains of extinct vegetables, we appear to contemplate the phyſical economy of a world long ago obliterated; but in thoſe of animals, we ſeem to behold ſomething that gives a glimpſe into the moral relations of thoſe beings of paſt ages who exiſted upon its ſurface.

Apparatus for Filtration
by Sir H. C. Englefield Bart. &c.

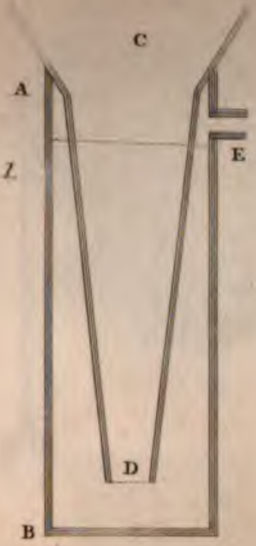
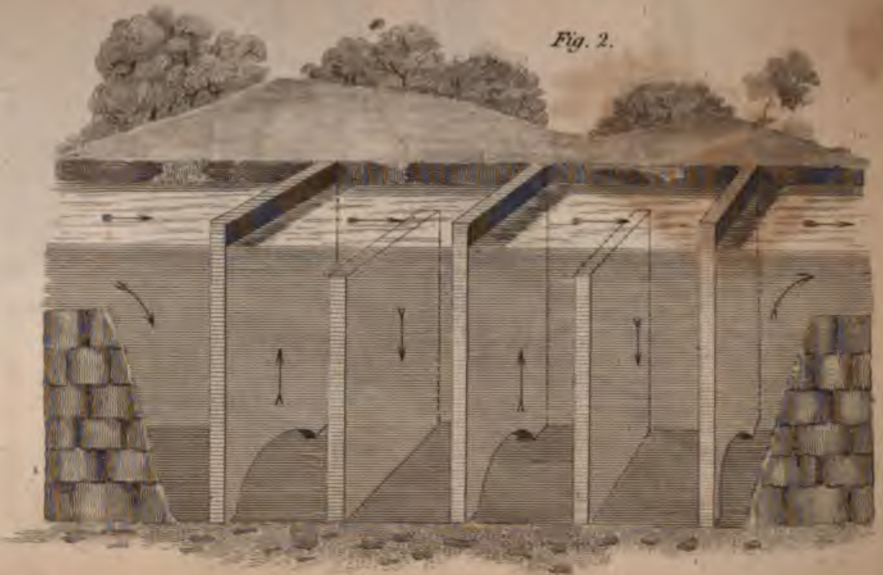


Fig. 1.

Structure for purifying a Stream
of Water.

Fig. 2.



Drawn by Blunt.

Engraved by Mutlow.

Striking part of a Clock, by Mr. Prior.

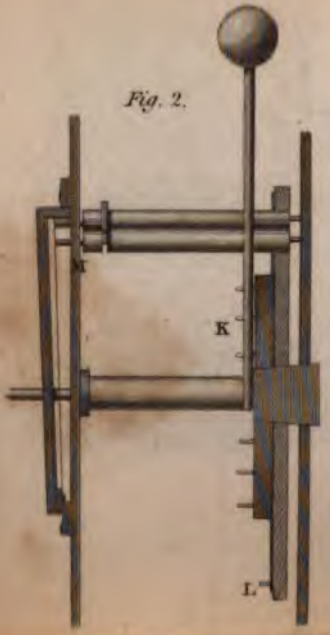


Fig. 2.

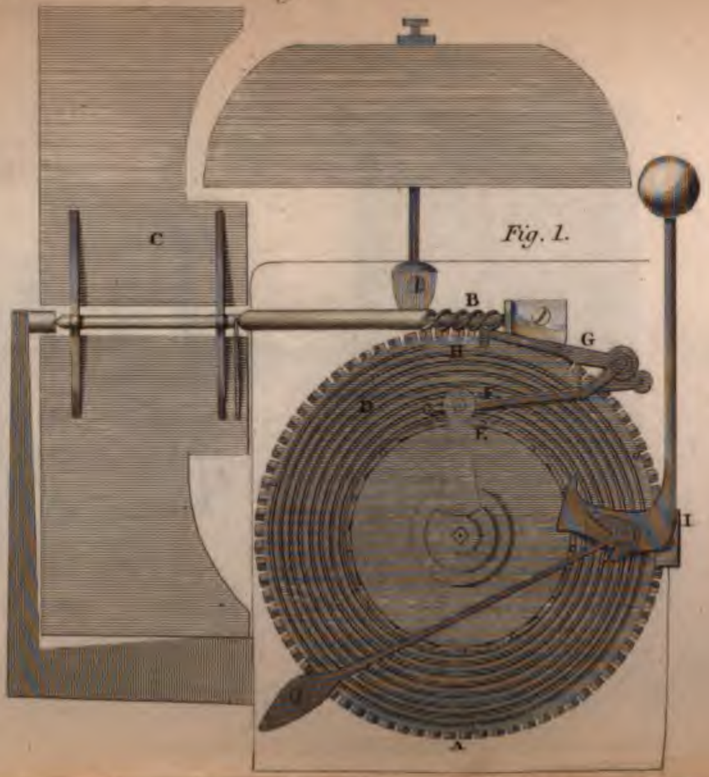


Fig. 1.

Mutlow Sc.

BLIC
ASTOR, L
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Mr. Antis's Indicator of the Number of Draughts from a Mine.

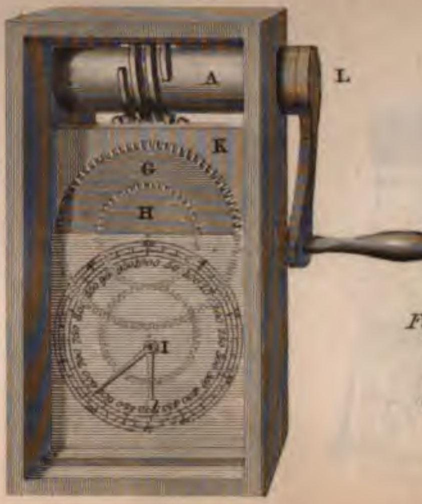

Fig. 2.

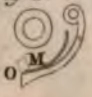

Fig. 3.

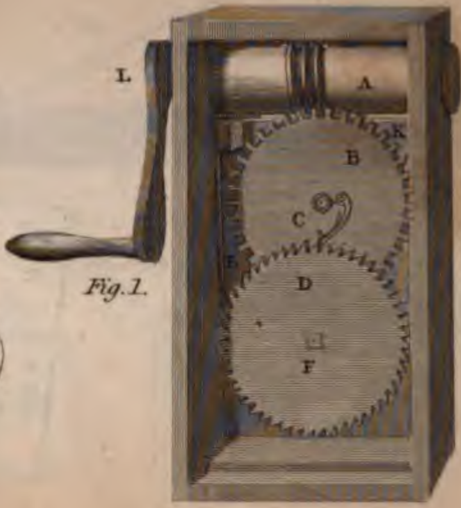

Fig. 1.

Mutlow Sc. Russell Ct.

Perspective · Instrument

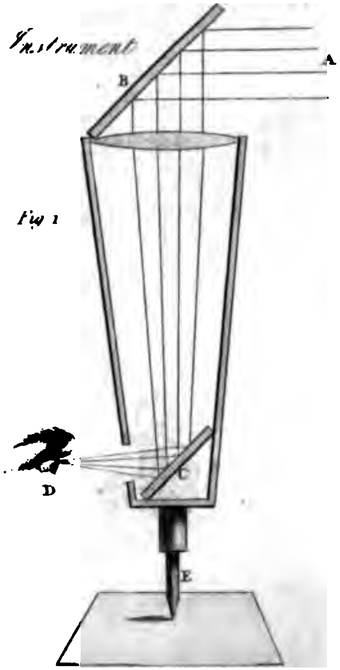

Fig 1

Instrument for drawing Ovals.

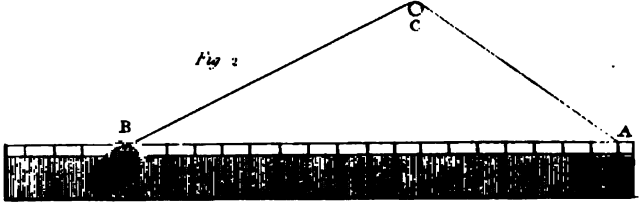

Fig 2

Mallens Port-folios.

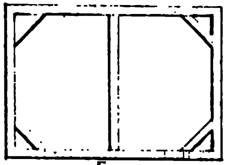

Fig. 3.

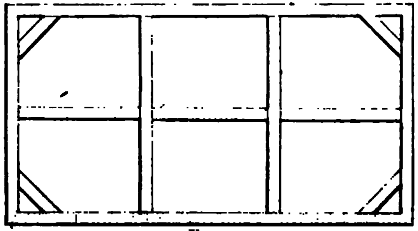

Fig 4.

A
JOURNAL
OF
NATURAL PHILOSOPHY, CHEMISTRY,
AND
THE ARTS.

NOVEMBER, 1804.

ARTICLE I.

Deſcription of a Tallow Lamp, which regulates its Supply by a ſpontaneous Movement. By Mr. JOHN WHITLEY BOSWELL. *Communicated by the Inventor.*

To Mr. NICHOLSON.

SIR,

THE conſtant attention which candles demand from the frequent ſnuffing they require, and the continued variation of their light in the intervals of this operation, on the one hand, and on the other the expence and trouble which oil lamps occaſion, together with the diſagreeable ſcent they in general produce, have rendered the contrivance of a good lamp for burning tallow, a deſirable object to ſeveral for a long time. Deſcription of a lamp for burning tallow.

I hope, therefore, the deſcription I ſend of a lamp for this purpoſe, which poſſeſſes ſome valuable properties, will be acceptable to you.

The beſt lamp for burning tallow which I had ſeen before I contrived this, was one invented by Mr. March, of Barnſtaple, in which the tallow kept continually melted in a veſſel ſuſpended over the flame, was admitted by a ſmall pipe to the burner, and the ſupply regulated by a kind of cock attached

Deſcription of a lamp for burning tallow.

to the pipe: But this method occaſioned a waſte from the evaporation of the melted tallow, and beſides, required much attendance to regulate the cock and keep the tallow veſſel at a proper diſtance from the flame; for the alteration of the temperature of the melted tallow, which was very frequent from many cauſes, continually changing its degree of fluidity, cauſed it to vary in its ſupply through the pipe proportionally.

' It occurred to me ſome time after I ſaw this lamp, that one might be contrived in which the tallow ſhould only be melted as it was conſumed, and other of the inconveniences be removed to which the firſt was ſubject; and I made then a rough drawing of a lamp for this purpoſe, but other more intereſting matters prevented my having one made on that plan before laſt December: It conſiſted of a trough ſupported in an inclination of about 45 degrees, ſimilar to the one in the figure, which held a ſquare priſm of tallow; under this was placed a little oblong veſſel of tin, formed ſo as to move forwards and backwards like a drawer, in front of which was placed a burner: a couple of wires placed acroſs the lower part of the trough, prevented the tallow from ſliding down beyond them, as it otherwiſe would from the obliquity of its ſupport; but as it melted by the heat from the burner, it fell in drops into the little drawer, ſo as to ſupply the flame, while the degree of this ſupply was regulated by moving the drawer in or out, ſo as to bring the burner nearer to, or further from the tallow. In this ſtate, though it formed a very uſeful lamp, it was not ſo perfect as I could wiſh; every alteration of temperature of the apartment (which in cold weather happens frequently) required a proportional alteration in the diſtance of the burner from the tallow, to prevent the little drawer being either overflowed by a ſuperabundance of melted tallow, or the flame ceaſing from a deficiency of ſupply. The attendance this circumſtance required induced me to contrive the preſent lamp, or rather to improve the former, in manner following.

I conceived that if the little pan, which held the melted tallow and the burner, was placed on the arm of a balance inclined from 30 to 45 degrees from the perpendicular, and its weight compenſated by a counterpoiſe at the extremity of the other arm, that when more than a certain quantity of tallow

Deſcription of a lamp for burning tallow.

low ran into it, it would ſink lower, and, in doing ſo, move the burner farther forward from the trough at the ſame time, and *vice verſa*, and thus regulate the ſupply ſpontaneouſly: But as it was neceſſary that the pan (holding a fluid) ſhould always maintain an horizontal poſition during its movement, I imagined this might be effected by placing another arm beneath the balance-arm, and fixing a perpendicular piece, ſo jointed to the extremity of each as to move freſh and ſteadily, which piece ſhould ſupport an horizontal ſtand for the pan.

I had a lamp made in this manner, and found it anſwer my intention perfectly. It required no attendance whatſoever, but regulated its ſupply with preciſion; and afforded, likewiſe, an agreeable ſpectacle, having in its movements ſomewhat the air of thoſe of an animal, from their exact relation to an evident object, and adopting themſelves to all its changes.

The lamp thus made aſſumed the form in the figure, Pl. IX. where T repreſents the inclined trough for holding the tallow; P the pan for catching the tallow as it melted; and holding the burner; F the flame and burner; R the perpendicular piece ſuſtaining a ſupport for the pan; B the balance moving freely on its center in the ſtand S, and jointed to R as before deſcribed; *c* the parallel arm uſed to keep R perpendicular, and by its means horizontal; *w* the counter weight, placed ſo as to move on the extremity of B by a ſcrew. The firſt froſty night ſhewed me the neceſſity of the addition of another part to remedy an inconvenience I had not foreſeen; the dropping tallow aſſuming the form of an iſicle, joined the pan to the trough, and thus ſtopped its movement: As it was troubleſome to break this off as it occurred, I contrived the following means to prevent its formation: A little oblong ſliding-piece, the ſame breadth as the trough, was placed cloſe underneath its lower part; the extremity of this ſlide was made angular, that the melted tallow might fall from a ſingle point; a wire repreſented at L, moved it back or forwards as required: By this ſlide the drop may be made to fall as cloſe to the ſtream of burning air from the flame as is thought fit, and all congelation prevented. I afterwards added a ſort of reflector to the lamp, which conſiderably improved the light, not merely acting as its name implies, but alſo occaſioning an

Deſcription of a lamp for burning tallow.

increaſe in the current of air which paſſed the flame, rendered it ſomewhat more bright, and conſiderably more ſteady. This part is repreſented at D in the figure, and conſiſts of two ſide-pieces placed before the trough, but ſo as not to obſtruct the tallow, and bent forward to form an angle with its ſides. To improve the light, I alſo uſe in the lamp five ſmall diſtinct wicks, of three threads each, inſtead of one large wick, placed near each other in a row, which renders the flame clear and free from ſmoke, and thereby alſo prevents any unpleaſant ſcent. I uſed this lamp for more than three months, and found it very convenient for reading or writing by, as when once lighted it required no farther attention, and kept its light at nearly the ſame height at all times, and of the ſame degree of intenſity; and I can further recommend it, as yielding the greateſt light at the ſmalleſt coſt, in proportion, of any invention yet made public which is applicable to domeſtic uſes.

I have had two lamps of this kind carefully made by Mr. Lloyd, No. 178, Strand, and left with him as models: In one of which the chief object is cheapneſs, as far as conſiſtent with perfection and neatneſs; in the other, to form an elegant utenſil for the ſtudy, office, or chamber; and any gentleman may be ſupplied with them there, made exactly to my plan, on reaſonable terms; and as Mr. Lloyd is the only perſon at preſent appointed to make them, and inſtructed fully in the mechaniſm, they can be had perfect from him only.

It is probable wax might alſo be burned in this lamp as well as tallow: of this I cannot ſpeak from experience, but mention it, as I know ſome would prefer to uſe it.

I am, Sir,

Your very humble ſervant,

J. WHITLEY BOSWELL.

Oct. 1, 1804.

Account

II.

Account of a ſucceſsful Caſe in which Deafneſs was cured by Punćture of the Membrana Tympani. By M. Maunoir, of Geneva. Communicated by ALEXANDER MARCET, *M.D. Phyſician to Guy's Hoſpital.*

To Mr. NICHOLSON.

SIR,

Introductory letter.

THE incloſed communication on the effects produced in a caſe of deafneſs by the perforation of the tympanum of the ear, is extracted from a letter which I have juſt received from Mr. Maunoir, an eminent ſurgeon at Geneva. The hiſtory of this caſe, drawn up by ſuch an intelligent and accurate obſerver, appears to me curious, both from the phyſiological remarks which it contains, and from the ſtriking illuſtration which it affords of a new mode of practice lately propoſed in this country. And, as this ſubject ſeems fully as intereſting in its philoſophical point of view, as in its connection with practical ſurgery, I ſhould wiſh, if you think it proper, that you would give it a place in the next number of your valuable Journal. I have the honour to be, Sir,

Your obedient humble ſervant,

ALEXANDER MARCET.

St. Mary Axe, Oct. 16, 1804.

Extract of a Letter from Mr. Maunoir, ſurgeon at Geneva, dated 5th September, 1804.

A gentleman aged 40 years had been ſeveral years extremely deaf.

"Mr. F. of Geneva, aged forty years, had loſt his hearing, ſeveral years ago, in conſequence of a long affection of the poſterior part of the internal fauces, and it was evident, that, in this caſe, the euſtachian tubes were obliterated. In condenſing the air in his mouth and noſtrils, he was totally unable to diſtend the membrane of the tympanum, a thing which is very eaſy to do for any one who enjoys a perfect ſenſe of hearing. He could ſcarcely hear, even when you ſhouted in his ears, and although ſo long deaf, he had not acquired the faculty of underſtanding by the motion of the lips. After peruſing the paper which I lately publiſhed in the Bibliothéque Britannique, Mr. F. conceived that he might be one of thoſe in whom the hearing could be recovered by the perforation of the

Puncture of the tympanum of one ear.

Inſtant reſtoration of hearing with extreme ſenſibility.

Some ſounds more audible than others.

The other ear punctured without effect.

Cauſe of this failure.

the tympanum. On the 14th of July laſt, I performed that operation on his right ear, in preſence of Mr. Jurine, with a very ſmall trocar, about one eighth of an inch in diameter. At the inſtant that the inſtrument was withdrawn, we ſpoke to him in a low voice; but inſtead of anſwering, he remained immoveable in his chair, and ſeemed ſtupified.—Then he exclaimed "For God's ſake gentlemen, don't cry out ſo loud; you hurt me!" I then began to walk about the room.—The ſqueaking of my boots made him ſhudder and jump in his chair; and he covered his ear with his hand. The ſnapping of the fingers quite diſtracted him, and ſeemed to produce upon him an effect ſimilar to that of a piſtol ſuddenly fired in the ear of a perſon not aware of it. When ſpoken to, though in a whiſper, he complained that we ſpoke too loud. Yet in ſpite of this exceſſive ſenſibility to very inconſiderable noiſes, there were other ſounds, not leſs diſtinct, which he did not hear, even at a very little diſtance; ſuch for inſtance as the *ticking* of his watch. So that his hearing ſeemed to be ſometimes too dull, ſometimes too acute; becauſe, no doubt, the organ had loſt the faculty of adjuſting itſelf to the different modulations of ſound. A week after this, however, Mr. F. had loſt that exceſſive ſenſibility which rendered acute ſounds almoſt intolerable to him, and in fact he had already *again learnt* to hear. He then wiſhed to have the other ear perforated in the ſame manner. I performed this operation, but without effect.—Twenty days after this ſecond operation, Mr. F. called upon me. I examined his ears in a fine ſun ſhine, and could ſee in the right ear the membrane of the tympanum traverſed in its exterior part by a ſmall cicatrix, having in its center a ſmall hole which was hardly perceptible. Yet the hearing was ſcarcely diminiſhed ſince the firſt operation. Fearing that this ſmall aperture might cloſe itſelf entirely, Mr. F. deſired that I would perforate again the membrane of that ear, an operation which was repeated without giving him any pain, but not without a ſlight increaſe of ſenſibility in his hearing. I afterwards examined the left ear, and having placed the meatus in the direction of the ſun's rays, I diſtinctly perceived a falſe membrane adhering to the whole circumference of the paſſage, and perfectly ſimilar, in its form and ſituation, to that of the tympanum, the diſtance between the two membranes being ſcarcely one ſixth part of an inch. I took off at once this falſe membrane with a pair of tweezers, and diſcovered behind it, the tympanum, quite ſound and entire. I thought it probable that, in the ſecond operation, I had only touched this falſe membrane. I immediately perforated the true one, and was ſurprized (as well as my brother who attended this operation), to find that, although Mr. F. had already learnt again to hear with the right ear, yet the reſtoration of his hearing in the left, occaſioned in him the ſame effects of wonder, and of exceſſive ſenſibility to the leaſt unexpected noiſe, which he had experienced after the firſt operation. This inconvenience, however, has not been of longer duration in this ear, than it had been in the other."

Repetition of the operation with effect and ſimilar phenomena.

III.

Letter from Mr. TIMOTHY SHELDRAKE, *expoſing the Errors of M. Tingry reſpecting Copal Varniſhes; with ſome farther Inſtructions concerning the ſame.*

(Received Oct. 16, 1804.)

To Mr. NICHOLSON.

SIR,

M. TINGRY, profeſſor of chemiſtry, &c. in the academy of Geneva, has publiſhed a treatiſe on the art of making and applying varniſhes; with new obſervations and experiments on copal, &c. &c. A tranſlation of that work has lately appeared here, and at p. 157 of that tranſlation I find the following paſſage.

Tingry in his art of making varniſhes—

" Mr. Timothy Sheldrake mentions camphor as a medium for diſſolving copal in eſſence[*] and alcohol. He gives alſo another proceſs, in which ammonia (volatile alcaline ſpirit of ſal ammoniac) in the proportion of an eighth part of the eſſence employed, is ſubſtituted in the room of camphor.

" *Exper.* I. Of the three proceſſes which he deſcribes, I repeated two: thoſe which regard the ſolution of copal in eſſence and in alcohol by a mixture of camphor. *That with eſſence did not ſucceed.* The author himſelf announces that he always

repeated Sheldrake's proceſſ for diſſolving copal in eſſence and did not ſucceed;—

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[*] Throughout the book this word is uſed to deſcribe what we call ſpirit of turpentine.

always failed, except when he obtained the eſſence from apothecaries hall. It appears that this eſſence had by chance all thoſe eſſential qualities which we endeavour to give it by time, and ſtill more ſpeedily by the influence of light.

ess with alcohol.

" *Exp.* II. The ſame experiment repeated with pure alcohol *was attended with too little ſucceſs to make the reſult be conſidered as a varniſh.* The alcohol appeared milky, and the copal formed at the bottom of the veſſel a maſs which did not ſeem to have decreaſed in volume. Next morning the interpoſed part of the copal, which altered the limpidity of the alcohol, was precipitated, and adhered to the ſides of the glaſs. The proceſs I have deſcribed for ſpirituous tincture of amber would give more hopes of ſucceſs without any intermediate ſubſtance."

He infers that the compounds muſt be imperfect and not varniſhes.

" In regard to the means propoſed by the medium of ammonia, the ſaline nature of that liquid, *if the proceſs ſucceeds,* will not admit of the product being placed in the claſs of varniſhes deſtined for delicate painting. *It is a kind of ſaponaceous compound, the uſe of which is not to be recommended in ſuch caſes.*"

Obſervations in reply.

The paſſage above quoted undoubtedly relates to a paper which I communicated ſeveral years ago to the Society for the Encouragement of Arts, &c.

This paper was wholly tranſcribed * into ſome periodical publications, and parts of it into others; it may therefore be neceſſary to obſerve that when reference is made to the original paper, it muſt be underſtood to mean as it was printed in the Society's Tranſactions, and not to any imperfect extracts that may have been made from it.

Mr. Tingry ought not to have oppoſed *ſpeculation* againſt *facts.*

Mr. Tingry ſhould have been ſenſible of the danger in oppoſing a conjecture or opinion *purely ſpeculative,* to what is deſcribed as a *fact that had been demonſtrated by experiment.* Since it is more probable that the conjecture ſhould be unfounded, or the opinion untrue, than that a man ſhould forfeit his reputation with the public by relating that, as having been demonſtrated by experiment, which the perſon who contradicts him ſays cannot, in the nature of things be true, even if the *proceſs ſucceeds:* As it is the intention of your valuable Journal to promote the cauſe of ſcience, and as that cauſe will be as effect-

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* See our Journal, I. 250.

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nally ſerved by the detection of errors, as by the promulgation of new facts, I truſt you will favour me by inſerting the following obſervations on thoſe paſſages which I have quoted from Mr. Tingry's work; beginning with that in which he attempts to demoliſh one part of diſcovery (if it may be ſo called) without uſing one experiment as the inſtrument of deſtruction.

" In regard to the means propoſed by the medium of ammonia, the ſaline nature of that liquid, *if the proceſs ſucceeds*, will not *admit of the product being placed in the* CLASS OF VARNISHES, deſtined for delicate painting; it is a kind of *ſaponaceous compound, the uſe of which is not to be recommended in ſuch caſes*."

Inſtances of pictures varniſhed with the copal many years ago, and now in perfect and excellent condition.

In oppoſition to all this I have but a ſimple fact to produce: viz. It is many years ſince I diſſolved copal by the proceſs alluded to; and actually varniſhed ſeveral pictures with the ſolution ſo prepared: thoſe pictures appeared to ſhine, or bear out, as artiſts call it, as well as if they had been covered with any other varniſh; the colour of the varniſh has not changed in the leaſt, which moſt other varniſhes would have done in the ſame time; they have ſtood in a ſmoky houſe in London the whole time, have been repeatedly waſhed with warm water, and cleaned with ſpirit of turpentine; and notwithſtanding all this, they ſtill preſerve their gloſſy appearance undiminiſhed. If a ſolution which is proved by the experience of many years to poſſeſs theſe properties in an eminent degree, does not deſerve to be admitted into the claſs of varniſhes, it is not eaſy to tell to what ſubſtance Mr. Tingry will grant that diſtinction.

The ammonia probably forms no part of the compound; but this is of no practical conſequence.

Having ſaid thus much for the fact, we may venture to proceed to the theory: When ammonia and copal were mentioned, the combination of both thoſe ſubſtances with the ſpirit of turpentine appeared to Mr. Tingry to be ſo much a matter of courſe, that he took it for granted, and drew all the concluſions from it without making a ſingle experiment to prove the reality of the fact, had he done ſo, even he, with the ſtrong prejudices he evidently has upon this ſubject, might have *doubted if the ammonia entered into the compoſition at all*; to me it appears, after attending carefully to the phenomena of the ſolution, that it *does not*; but without determining poſitively on the ſubject, I ſhall deſcribe thoſe phenomena, and leave your readers to judge for themſelves.

Particular deſcription of the proceſs for copal in ſp. of turpentine by ammonia.

The copal muſt be groſsly powdered and put into a convenient glaſs veſſel; the ſpirit of turpentine and ammonia muſt be well ſhaken together, and the mixture poured upon the copal; the veſſel cloſed, and ſo much heat applied as ſhall, as ſpeedily as poſſible, make the ſpirit boil ſo ſlowly that the bubbles may be counted as they riſe to the ſurface; if this is ſkilfully performed, the ſolution is ſeen to take place in the following manner; the mixture has a milky appearance when firſt applied to the copal; the upper ſurface firſt becomes clear, the cloud gradually ſubſides, and attaches itſelf to the copal, which ſwells and becomes opaque; a portion of it in a diſſolved ſtate gradually exudes with the air bubbles, and diffuſes itſelf in the ſpirit, and this continues till it has taken up as much as can be ſuſpended in it; no more is then diſſolved: It is on the ſkilful management of theſe circumſtances that the ſucceſs of the proceſs depends; for, if leſs heat than the degree required be given, the ſolution ſtops; if more, the bits of copal inſtead of being opaque and adhering together at the bottom of the veſſel, reſume their tranſparency, and are agitated ſeparately by boiling of the ſpirit; and when either of theſe accidents happened, I could never afterwards diſſolve the ſame portion of copal either in the ſame or any freſh portion of ſpirits. If I might venture to explain theſe phenomena, I ſhould ſay, that the ammonia gradually quits the ſpirit of turpentine, and unites with the copal, and reduces it to ſuch a ſtate, that with the aſſiſtance of the peculiar degree of heat that I have deſcribed, the ſpirit of turpentine is enabled to diſſolve ſo much as will form a varniſh; but, if either more or leſs heat is applied, the copal cannot be diſſolved, and then the operation is abortive.

The ſucceſs depends on the management of the circumſtances.

Excellence of this varniſh, &c.

Whether this explanation be ſatisfactory or not is of ſmall conſequence, the exiſtence of pictures in my poſſeſſion, which were varniſhed with this ſolution many years ago, and ſtill remain more perfect than they would have been if covered with any other varniſh, is ſufficient to prove that all Mr. Tingry has ſaid on this ſubject has not the ſmalleſt foundation in truth.

Hint which produced the diſcovery.

I had examined all the books which I had acceſs to, without getting any information reſpecting the ſolution of copal for the purpoſe of making varniſh, till in Lewis's tranſlation of Neuman's works I found the following, Vol. II. p. 27. " Solutions of copal have been greatly eſteemed as varniſhes, and the method of making the ſolution kept a ſecret in particular hands;

Junker informs us that it *readily ſucceeds, if ſpirit of ſal-ammoniac, mixed with a due proportion of oil of ſpike or oil of turpentine*, be uſed as the menſtruum." Upon this tint I proceeded till I had diſcovered the proceſs I have deſcribed; but, though the varniſh anſwered every purpoſe I could wiſh, the trouble of making it being great, and the failure of the proceſs ſo frequent that I determined to ſeek for ſome more convenient method.

Invention of camphor as a better medium than ammonia.

Having found that copal, when powdered and rubbed in the mortar with a ſmall quantity of camphor, became ſoft, and in a few minutes, united in one coherent maſs, I conjectured that by means of camphor, copal might be diſſolved in alcohol, and upon trial found that it might be ſo diſſolved, and rendered fit for the purpoſes of varniſh; this I did, not by way of experiment, but in conſiderable quantities and at various times; ſo often, that I am authoriſed to ſay, that if the materials are good, and the proceſs I have deſcribed in the Tranſactions of the Society for Encouragement of Arts be followed, there can be no difficulty in making a colourleſs ſpirit varniſh from copal; and as a proof that this may be done, I ſend you a ſmall quantity that I made ſome time ago, and which will enable you to judge whether it deſerves to be "*placed in the claſs of varniſhes.*"

Sp. of turpentine preferable to alcohol.

But as it is improper, for many reaſons, to varniſh pictures with any ſubſtance diſſolved in alcohol, I tried to diſſolve the copal in ſpirit of turpentine by the intervention of camphor, and ſucceeded; not ſo eaſily indeed as in alcohol, but always with the certainty of ſucceſs, when the precautions I have deſcribed were obſerved; this has been uniformly the caſe with myſelf for ſeveral years, as well as ſeveral artiſts who have made the trial; and it has now been proved by many years experience that copal diſſolved, by means of camphor, in ſpirit of turpentine, is as colourleſs as the reſin itſelf and perhaps as durable, ſince pictures that have been varniſhed many years with it have been preſerved without the leaſt change even in the colour of the varniſh, which would not have been the caſe had any other ſubſtance been uſed.

This varniſh is alſo excellent.

As Mr. Tingry's *experiments* lead to very different concluſions, I am compelled to ſuppoſe, either that they were inaccurately made, or that the copal, camphor, alcohol, and* ſpirit

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* Mr. Tingry himſelf, *as he ſays*, tried to diſſolve copal in ſpirit of turpentine, and the proceſs did not ſucceed: his notion that the

ſpirit of turpentine in his country are very different from the ſubſtances which are ſold by the ſame names in this.

General remarks.

My intention in communicating thoſe papers to the Society was to aſſiſt thoſe who are engaged in the practice of the arts, by making public a method of making a varniſh that muſt be valuable to them: and I have been induced to trouble you with this, in hopes that, by printing it, you may prevent thoſe falſe notions, which might be formed upon Mr. Tingry's experiments from gaining credit; to which you will allow me to add ſuch hints and improvements as may prevent diſappointment to thoſe who undertake to prepare this varniſh for themſelves inſtead of depending upon thoſe who make it for ſale; this every artiſt ſhould do if he reſolves to have it perfect; for, though I do not mean to ſuppoſe that every maker of varniſh will ſubſtitute inferior articles, yet it is certain that where the price of one article is higher, becauſe it is really valuable, and difficult to make, many dealers will ſubſtitute ſomething *inferior* that produces more profit to himſelf, without conſidering whether it may not be injurious to thoſe who are to uſe it.

Narrative of an artiſt who did not ſucceed in the proceſs.

I had ſeveral times given ſome of my varniſh to an artiſt, who liked it ſo well that he requeſted I would inſtruct him to make it himſelf, inſtead of making frequent applications to me for it: I procured him the neceſſary apparatus, and gave him a written account of the proceſs, with ſuch verbal inſtructions as I could add to it, and left him to procure his own materials and proceed to work by himſelf; he did ſo, and did not ſucceed in diſſolving the copal; he tried a ſecond and third time with no more ſucceſs; he then brought his materials to me, requeſted that I would try if I could diſſolve them; which experiment would determine whether his materials were bad, or whether there was any defect in his manner of attempting the proceſs: this I reſolved to undertake; uſing the ſame ſpirit

ſpirit I uſed had *by chance* obtained thoſe powers for diſſolving copal which *he endeavours to give it by time*, &c. is perfectly ridiculous. You and your Engliſh readers will know that in ſaying I did not ſucceed when I uſed ſpirit of turpentine from the colour ſhops, but always did when I got the ſpirit from Apothecaries Hall, I meant that at the latter place I always obtained the beſt article of the kind, inſtead of the inferior articles which are ſold by the *ſame name* in other places, though totally unfit for this purpoſe.

and copal which he had thrice attempted in vain to diſſolve; and at the ſame time ſubjecting another portion of my own materials to the ſame proceſs in every particular: I ſucceeded in both inſtances: the only difference was that the ſolution which I produced from the materials which had been tried by my friend was not ſo viſcid as that which was obtained from the freſh materials.

Improved proceſs.

As the proceſs which I have uſed for ſome time paſt is different from and better than that which I communicated to the Society, and has been publiſhed in their Tranſactions, I truſt you will excuſe me for deſcribing it here; it is ſuch an one as may be ſucceſsfully uſed by any perſon upon a common fire, without any danger, and which any man of ſcience, or who may chuſe to adopt in a large way, may eaſily modify ſo as to anſwer his purpoſe.

Safe and eaſy method of making copal varniſh.

Provide a ſtrong veſſel made of tin or other metal, it ſhould be ſhaped like a wine bottle and capable of holding two quarts; it will be convenient to have a handle ſtrongly rivetted to the neck; the neck ſhould be long and have a cork fitted to the mouth, but a notch or ſmall hole ſhould be made in the cork, that, when the ſpirit is expanded by heat, a ſmall portion may force its way through the hole, and thus prevent the veſſel from burſting.

Diſſolve half an ounce of camphor in a quart of ſpirit of turpentine, and put it into the veſſel; take a piece of copal the ſize of a large walnut, reduce it to coarſe powder or very ſmall pieces; put them into the tin bottle, faſten the cork down with a wire, and ſet it as quick as poſſible upon a fire ſo briſk as to make the ſpirit boil almoſt immediately; then keep it boiling very gently for about an hour, when ſo much of the copal will be diſſolved as will make a very good varniſh; or, if the operation has been properly begun, but enough copal has not been diſſolved, it may be again put on the fire, and by boiling it ſlowly for a longer time, it may be at laſt brought to the conſiſtence deſired.

Explanation of the proceſs;

The rationale of the operation I believe will be nearly as follows: copal and camphor have ſo ſtrong an affinity for each other, that when ſeparately powdered and then rubbed together, the copal abſorbs the camphor, ſwells, ſoftens, and becomes one coherent maſs; nor when once united do they eaſily ſeparate again *. When the camphor is diſſolved in the ſpirit, the ſmall bits of copal thrown into it and inſtantly made to boil, the motion of ebullition keeps the camphor ſuſpended, moves the copal about in the ſpirits, the ſurface of each bit of copal is diſſolved, and the whole of it ſoftened and enlarged, and by continual boiling may be diſſolved; but my friend did not proceed in this way; he reduced his copal to a very fine powder, thinking, perhaps, that would facilitate the ſolution; he then added the camphor, diſſolved in the ſpirit of turpentine, but fearſul of giving too much heat, ſet it by the ſide of his fire till it was warmed, then moved it into a warmer place, and at laſt made it boil; the conſequence was that the camphor † left the ſpirit and united with the copal to form one ſolid coherent maſs at the bottom of the veſſel; the thickneſs of this maſs prevented the heat from penetrating to the ſpirit ſo ſoon as it otherwiſe would have done; and, when at laſt it did boil, the camphor had entirely left it, and of courſe it was impoſſible that any ſolution ſhould take place.

—and why Mr. S.'s friend failed.

Three times was the ſolution attempted in this manner, but when on a fourth trial by myſelf a ſufficient quantity of camphor was added and the boiling heat immediately applied, the ſolution went on, but not ſo favourably as in my other attempt, becauſe the copal was united into one maſs at the bottom of the veſſel, and no more could be expoſed to the action of the ſolvent than the ſurface; but, in the other experiment the copal was broke into very ſmall bits and the ſurface of each expoſed to the full action of the ſolvent; of courſe a greater quantity was diſſolved in the ſame ſpace of time.

The inſtance of failure here mentioned might have been urged againſt the proceſs.

Had this gentleman been ſatisfied with the experiment he tried by himſelf, he might have reported, from his own experience, that copal could not be diſſolved by the proceſs I had deſcribed; and yet in point of fact, nothing could have been more untrue: from this it may be fairly deduced that when perſons who are unuſed to ſuch experiments do undertake them they ſhould be ſcrupulouſly exact in following the directions given

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* Six months ago I reduced ſome copal to this ſtate; it has remained ever ſince expoſed to the open air in a hot room, and ſtill remains ſoft, the particles adhering to each other.

† When the ſpirit of turpentine was poured off from the copal, it was perfectly colourleſs, and by the ſmell did not in the leaſt indicate the preſence of camphor.

given to them, and on any apparent failure, reſort to thoſe whoſe experience may enable them to detect any fallacy there may be in the proceeding.

Remarks and cautions.

As it is well known that ſolution of copal is the moſt valuable of thoſe varniſhes which can be uſed for the finer works of art, and as it is proved that by the proceſs I have deſcribed, theſe ſolutions may be eaſily made by any one in almoſt any ſituation, it only remains to mention ſome cautions which are neceſſary to enſure ſucceſs.

Firſt, That the ſpirit and camphor ſhould be united before the copal is added.

Secondly, That the copal ſhould be groſsly powdered before it is added to the ſpirit, the whole ſhould then be made to boil as *quickly as poſſible*, but not *violently*, and that ebullition ſhould be kept up regularly till the ſolution be compleat.

Effects of an overdoſe of camphor or of copal.

There ſeems to be no diſadvantage in uſing too much camphor if it ſhould ſo happen; but it *is* diſadvantageous to put in too little copal; becauſe a certain quantity of ſpirit will take up a given quantity of copal; and if more is added than will be diſſolved, the ſuperabundant copal will abſtract more camphor from the liquid, and thus render the ſolution more tedious and difficult, and ſometimes, perhaps, entirely prevent it.

Glaſs veſſels and thoſe of metal compared.

The advantage that attends the uſe of glaſs veſſels is that the ſolution may be ſeen all the time it is going on; the diſadvantage is that perſons who are not accuſtomed to ſuch purſuits, may inadvertently give too much heat, and thus ſubject themſelves to ſerious accidents; but by uſing veſſels of metal with the precautions I have given, no accident can happen; the only diſadvantage is that they cannot ſee what they are doing; this is of ſmall conſequence, as it may always be determined with ſufficient accuracy by the ear whether the ſpirit boils, and to what degree; and as no accident can happen, without very groſs neglect, from the uſe of them, they certainly ſhould be preferred.

Concluſion.

As I have written more than I at firſt intended, I ſhall conclude with a requeſt that you will uſe your diſcretion in omitting any part or rejecting the whole of what I have written, if you think the publication will not anſwer any uſeful purpoſe. I ſend with it a ſmall quantity of copal diſſolved in alcohol, and another in ſpirit of turpentine, which will enable you to ſatisfy yourſelf and your readers whether theſe ſolutions *deſerve to be placed in the claſs of varniſhes*, and remain

Your moſt obliged ſervant,
T. SHELDRAKE.

No. 5, Montague-Street,
Ruſſel-Square.

IV.

Reply to Mr. Dalton, *on the Conſtitution of Mixed Gaſes.*

By Mr. John Gough.

To Mr. NICHOLSON.

SIR, *Middleſhaw, Oct.* 16, 1804.

Character of Mr. Dalton's reply.

I DID not attack Mr. Dalton's opinions openly, until he had invited me to do it; the attempt has been made on my part purſuant to his requeſt; and he has replied, but in a manner which ſeems better calculated to amuſe the ſuperficial reader, than to convince the reaſoner: For he treats the ſubject, ſometimes with acrimony, and ſometimes with ridicule; in the mean time his arguments are but few, and theſe appear to be negligently conducted. A bare inſpection of his own letter will ſupport the two firſt charges, and I will now enter upon the defence of the third.

Mr. Dalton begins by pronouncing the diſtinctions which I make betwixt a mechanical hypotheſis and theory, to be nugatory; notwithſtanding the cauſes of theſe diſtinctions are fully ſtated in my Strictures. Now controverſy is of a nature which always obliges a man to point out the fallacy or futility of his opponent, when he can do it with ſucceſs; but this has been neglected by Mr. D. in relation to my diſtinctions, and I leave him to draw the inference which reſults from theſe obſervations.

Mr. D.'s demonſtration reviewed.

Mr. D. proceeds, in the next place, to demonſtrate the fundamental propoſition of his hypotheſis; namely, the mutual penetrability of gaſes which do not attract nor repel each other. For this purpoſe he remarks, that oxigen repels oxigen, but not azote. This aſſumption is fairly allowed to be a poſtulate by Mr. Dalton; and he then proceeds to obſerve, that this being admitted, it follows, that if a meaſure of oxigen be put to one of azote, the oxigen finding it, *viz.* the azote, porous, enters its pores, and *vice verſa*, &c. But here a ſecond and an eſſential poſtulate is ſlily introduced into the ſyllogiſm, under the form of an inference; becauſe no ſtrict logician will venture to ſay that a body, A, muſt be porous of neceſſity, becauſe it repels a body B; on the contrary he muſt conclude, that this miſtake in his art deſtroys the demonſtration, and does not retrieve the doctrine from the imputation of being an hypotheſis. The ſimile of the philoſopher, cottager, and ſieve, may be calculated to promote ridicule, but it is badly choſen; for it is impoſſible to demonſtrate the propoſed problem from Euclid's Elements, becauſe this work confines its ſpeculations to abſtract figures and magnitudes. But a mathematician might demonſtrate the ſame to a brother mathematician, from the elements of the mechanical philoſophy; and he ought to have his demonſtration ready upon demand, which is not the caſe of Mr. D. in the inſtance of his fundamental propoſition.

Mr. D.'s ſimile incorrect.

It is a matter of ſome ſurpriſe, that Mr. D. has drawn his demonſtration from the mutual affections aſcribed by him to oxigen and azote; ſeeing I had profeſſedly attempted to ſhew the fallacy of his ſecond and eſſential poſtulate, by aſcribing the ſame affections to aqueous vapour and the permanent gaſes of the atmoſphere. This unfortunate choice obliges my opponent to be content with a diſtant ſide-view of the arguments which have been brought againſt him; whereas a ſtricter obſervance of the path* chalked out for him in my Strictures, would have given him an opportunity to confront them, and defend his atmoſphere of ſteam. The thing however has turned out, for ſome reaſon or other, as it is here repreſented; and Mr. D. arrives very briefly at the following ſingular concluſion; that though the demonſtration of my propoſition reſpecting air and vapour, be rigid, the previous data are not correctly aſſumed. Now this is the point which I endeavoured to eſtabliſh in the propoſition alluded to above; for his ſecond poſtulate makes one of the incorrect data; in fact, they are all

The ſecond poſtulate falſe in the inſtance of vapour and air.

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* Phil. Journ. Vol. IX. Page 53.

his own except this: If a particle of vapour can paſs freely through the air, a ſecond can alſo ſucceed it at any given diſtance. My antagoniſt will certainly grant me this conceſſion; becauſe the ſalvation of his aqueous atmoſphere obliges him not to ſuſpect it. Should this indulgence be extended to my Strictures, the demonſtration in queſtion muſt be re-examined with greater diligence by Mr. Dalton; for if it cannot be refuted, it will manifeſtly do away his atmoſphere of free vapour, by proving the abſurdity of his ſecond poſtulate in the caſe of air and water; and the ſame obſervation may alſo be extended to all the permanent gaſes which are not abſorbed by water; becauſe no fluid of this deſcription forces water to paſs through its pores by preſſing upon it.

The uſe of the contemplative analyſis.

After all, if we may judge from Mr. Dalton's reply, he appears not to comprehend my arguments on this ſubject: they have however nothing of novelty in them; for the beſt reaſoners in all ages have not heſitated to admit a hypotheſis is for the ſole purpoſe of diſcovering its worth, by comparing the conſequences reſulting from it with known facts. This kind of argumentation was in great eſteem with the Greek mathematicians, and Pappus of Alexandria has deſcribed and recommended this ſpecies of logic under the name of the contemplative analyſis.

Facts oppoſing the hypotheſis.

Mr. Dalton ſays, he is acquainted with no facts that contradict what I call his experimental probabilities. I can furniſh him with one to which perhaps he is a ſtranger, and remind him of another, which he will not diſpute: it will therefore be his buſineſs to reconcile them to the hypotheſis of an aqueous atmoſphere.

An experiment by Mr. Kirwan.

Firſt, Mr. Kirwan, in the courſe of his ſtatical experiments on air, conſtantly found a given bulk of it to be lighter, *cæteris paribus*, when De Sauſſure's hygrometer was at 90°, than when the atmoſphere was leſs humid. Now according to Mr. D.'s hypotheſis, all the permanent gaſes, of this given bulk, had the ſame weight both in the dry and moiſt ſtate of the air; becauſe they were of the ſame denſity on account of the *cæteris paribus*: but the aqueous vapour was moſt abundant when the weight of the aggregate was leaſt; that is, the weight of vapour diminiſhes, while its denſity increaſes. Will Mr. D. admit the truth of the preceding concluſion, to the detriment of his own doctrine; or will he refute the inference, by denying the fact?

An experiment with a moiſt bottle.

Second, If the latter alternative ſhould be preferred, the following experiment, with which Mr. D. is already acquainted, remains to be explained by the principles of his hypotheſis, due attention being at the ſame time paid to the axioms of Dynamics.

A moiſt bottle, which I had found would contain 7794 grains of water of the temperature of 59°, was placed in water heated to 126°, having its mouth elevated about two inches above the ſurface. This aperture was covered by my hand; which was occaſionally removed for an inſtant, that the warm air might eſcape. At the end of two minutes the bottle was inverted in cold water, the mouth of it being firſt ſecured in the manner deſcribed above; in which ſituation the temperature of it was again reduced to 59°. The air remaining in it after refrigeration, occupied 6172 of the 7794 parts conſtituting the capacity of the bottle. From this fact I infer, that 6172 parts of air of 59°, will occupy 7794 ſuch parts after being raiſed to 126° in contact with moiſt glaſs. But 6172 parts of dry air of 59°, will only occupy 6992.06 parts, after being heated to 126° in a dry tube: Thus it appears that the preſence of water augmented the bulk of the air which I uſed; how is this difficulty to be explained? We have no ſtopple of mercury in this experiment, by help of which Mr. D. explains the appearances of the manometer; conſequently, as nothing was in the way to prevent the free egreſs of the vapour, it expanded itſelf in the bottle, and acted on the air contained in the ſame, juſt as it does in the atmoſphere. But it increaſed the bulk of this air, which could not be effected on Mr. D.'s principles, otherwiſe than by diſtending the pores of it. Now his own tables of the force of vapour will ſhew this to be impracticable, if there be any truth in the axioms of Dynamics. On the contrary, nothing occurs in the two laſt experiments repugnant to chemical union; and as water makes a part of the atmoſphere, it muſt be attached to air by the mediation of affinity.

Recapitulation.

I have now done ſomething more than anſwer all Mr. Dalton's objections: the demonſtration of his fundamental propoſition has been ſhewn to be no demonſtration at all; his ſecond poſtulate has been proved to be falſe, by his own data, in the caſe of air and vapour; laſtly, two facts have been advanced which are inexplicable by my opponent's principles:

ciples: After peruſing theſe objections, can he blame me for rejecting his hypotheſis. As for Mr. D.'s remarks on my theory of mixed gaſes, they require no anſwer; becauſe he profeſſes not to underſtand it. I am willing, however, to ſubmit the merits of it, as well as its claim to the title of a theory, to thoſe mechanical philoſophers who happen to comprehend it.

JOHN GOUGH.

V.

On the apparent Size of the horizontal Moon. In a Letter from Mr. Ezekiel Walker.

To Mr. NICHOLSON.

DEAR SIR,

Enlarged appearance of the horizontal moon.

WHEN the full moon is juſt riſing above a clear horizon, ſhe is an object that pleaſes every eye: But at the ſame time that ſhe pleaſes the eye of the philoſopher, ſhe embarraſſes his reaſon, to aſſign the cauſe of her apparent magnitude's being ſo much greater near the horizon, than at higher elevations.

Attempts to account for it.

When men in former ages began to reaſon on this phenomenon, they imagined that the angle ſubtended by the moon, was really increaſed, by a refraction of her rays, in paſſing through the vapours contained in the air, near the earth's ſurface. But when it became known that ſhe ſubtended the leaſt angle at the eye, when her apparent magnitude was greateſt, philoſophers ſaid that the eye was impoſed upon by the long ſeries of objects interpoſed between the eye and the extremity of the ſenſible horizon. And after it was diſcovered that the ſame phenomenon was obſerved at ſea, where no land objects could be ſeen, they blamed the clouds for deceiving them; and when the clouds flew away, the ſpirit of inquiry flew after them, to ſeek for information in the apparent concavity of the ſky.

Such were the erroneous opinions maintained by men of the greateſt celebrity, both in ancient and in modern times. To mention any more of them would be uſeleſs labour: a ſingle quotation from Dr. Smith's Optics will, I preſume, be quite ſufficient to ſhow how little was known of this matter at the time when he wrote.

After the Profeſſor had finiſhed his celeſtial explanation of this phenomenon, he juſtly acknowledges that, at different times, the moon appears of very different magnitudes, even in the ſame horizon, and occaſionally of an extraordinary large ſize, which he is not able to give a ſatiſfactory explanation of. *Smith's Optics*, Vol. I. pa. 63, &c. *Remarks*, pa. 53.

It is really aſtoniſhing, that this phenomenon ſhould have remained ſo long without an explanation founded on a better principle than that of mere opinion. That the dimenſions of the pupil of the eye alter by the ſtimulus of light, has long been known; but I believe it ſtill remains to be proved, that the picture of the moon formed upon the retina is not permanent, but varies as the dimenſions of the pupil vary. The following experiments will, I hope, remove every doubt reſpecting this law of viſion.

Explanation offered upon the poſition that the images on the retina vary with the ſize of the pupil.

To imitate the eye upon a large ſcale, I took an achromatic lens of 1.6 inches in diameter, and 17 inches focal diſtance, to repreſent the cryſtalline humour. This I fixed upon a ſtand in a perpendicular direction, with a moveable white ſcreen behind it for the retina, to receive the image of a lighted candle, which ſtood before it, at the diſtance of 40 inches. To imitate the pupil, I took three cards, and numbered them 1, 2, and 3, In No. 1, I made a circular hole half an inch in diameter, in No. 2 a hole of $\frac{3}{10}$ of an inch, and in No. 3, a hole of $\frac{2}{10}$ of an inch.

Experiment. A candle was placed before an achromatic lens, and three apertures were provided.

After I had moved the ſcreen until the image of the candle appeared diſtinctly upon it, I found that the length of this luminous picture meaſured 1.82 inches.

Focal image by the whole lens 1.82 inches long.

When the card No. 1, was applied cloſe to the lens, with the centre of the hole againſt the centre of the lens, the inverted image of the flame of the candle, upon the ſcreen, meaſured 1.46 inches.

Half an inch aperture gave an image of 1.46 inch.

When the card No 2, was applied to the lens, in the ſame manner as No. 1, the image of the flame meaſured 1.3 inches.

Three tenths gave 1.3 inch.

And with No 3, the picture of the flame meaſured only 1.2 inches.

Two tenths gave 1.2 inch.

Theſe experiments ſhow us, in the cleareſt manner, the cauſe of that variation which obtains in the apparent magnitudes of objects.

Application of the experiments to the moon and other objects,

That the pupil contracts as the quantity of light which falls upon the eye increaſes, is well known.

And that the magnitude of the picture of a luminous object upon the retina, decreaſes as the pupil contracts, is evident from the experiments.

Therefore, the magnitude of the picture upon the retina, decreaſes, as the quantity of light falling upon the eye increaſes.

Hence the apparent magnitude of the moon is greateſt at the horizon, and decreaſes as ſhe aſcends; for the magnitude of her picture upon the retina is inverſely as the quantity of light which ſhe gives us.

and to the ſun.

The ſun appears larger at the horizon than at higher altitudes, for the ſame reaſon; and terreſtrial objects ſeen through a miſt appear larger than in a clear day, in conſequence of the ſame operation of the eye.

I am, Dear Sir,
With much reſpect,
Your humble Servant,
E. WALKER.

Lynn, Oct. 13, 1804.

VI.

Deſcription of the Ship Economy, 200 *Tons Meaſurement, built on the improved Conſtruction of Mr.* J. W. Boswell. *

THE plan adopted in the formation of this ſhip is that deſigned for large ſhips of 500 tons and upwards, and the third mentioned in my ſpecification †.

Its external appearance is nearly the ſame as that of any other veſſel of the ſize, and the outward planking done in the uſual manner. It is the internal conſtruction alone to which the patent relates, and that is as follows:

Deſcription of a veſſel framed in a new method.

The beſt general idea of it will be obtained by conceiving a veſſel built with timbers, or ribs, much ſmaller than uſual, with an internal framing, ſo contrived as to give every requiſite ſupport and ſtrength both to them and the entire veſſel, with the leaſt timber, and of the cheapeſt form, and without any knee-timber.

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* For which he has a patent.

† See Vol. II. Second Series, p. 81, of Repertory of Arts.

Deſcription of a veſſel framed in a new method.

The floor-timbers are molded ſeven inches, and ſided ſix: theſe, with four futtocks and two top timbers at each ſide, form what is called a frame of timbers. Thoſe ſmall timbers are laid down ſo that their terminations all fall out in fair lines, which are nearly the ſame as the ribband lines, when below the wales. Along thoſe lines inſide are laid fore and aft ribs, from ſtem to ſtern poſt, ſo as to ſupport the extremity of every one of the ſmall ribs in the ſhip. The fore and aft ribs are ſix in number at each ſide; one directly under the water ways, another at the level of the lower beams, and the other four placed nearly at equal diſtances between theſe laſt and the kelſon: each pair uniting in a breaſt-hook at the ſtem.

The pieces of timber which form theſe fore and aft ribs are ſcarfed at their extremities with hook ſcarfs, and ſo placed that the ſcarfs fall out in fair vertical ſections of the ſhip, where they are ſupported, and firmly bolted to tranſverſe framings, contrived ſo as to unite the greateſt ſtrength with the leaſt obſtruction, and which are five in number in the whole ſhip.

Thoſe tranſverſe framings laſt mentioned muſt be conſidered as the great ſupport of the veſſel, and the foundations, as it were, on which all the other parts reſt, as the beams of a wooden bridge are ſupported by the piers. Thoſe tranſverſe framings are each formed by one upper and one lower beam, two pair of futtocks, a floor timber, two pair of top timbers, and four bracing pieces; the whole connected into one firm framing, ſelf-ſupported, independent of any other part.

The four bracing pieces form each framing into a ſet of triangular compartments: which triangular framing gives the greateſt ſtability poſſible, as a triangular frame cannot be made to give in, or alter its figure, by any force which is not ſufficient to tear its connecting parts through the timber of which it is compoſed; a property which no other figure poſſeſſes.

The leading principle is that triangular framings cannot vary.

Theſe tranverſe framings (beſides ſupporting the fore and aft ribs, and by them the ſmall vertical timbers) tie and unite the veſſel together acroſs ſhip, ſo as to give much greater ſtrength than hanging-knees, whoſe place they ſupply, at a much cheaper rate.

The

Framing of the deck.

The framing of the deck is alſo divided into triangular compartments, as ſpecified in my patent, as to preclude the uſe of lodging-knees entirely; which compartments are formed by ſix pieces of timber, which proceed obliquely at each ſide, from the top of each beam to the fore and aft rib next adjoining, into which they are dovetailed and bolted; long carlings from beam to beam, at each ſide of the hatchways, with theſe pieces, ſupport ſmall ledges, on which the deck is laid in the uſual manner.

The veſſel in coming round from Southampton water ſailed remarkably faſt, and ſtayed and ſteered admirably well.

Advantages of this Method of framing Ships.

Advantages of the method:

1ſt. Timber of leſs than one fourth of the uſual girth can be uſed, in this method, in conſtructing large veſſels, for nearly four-fifths of their frames.

This will be a direct ſaving in the difference of price of ſmall timber and large for the quantity uſed; for large veſſels this will be conſiderable, and, according to the preſent contract prices for naval timber, not leſs than from two to four pounds per load. Beſides this, it is a great national benefit in another point; for, by this means, timber of half the number of years growth, or leſs, can be uſed for naval purpoſes; and thus forty or fifty years, or even leſs, be ſufficient to produce timber fit for the navy, inſtead of the vaſt period of near a century, now neceſſary; by which the land will not only produce a double crop in the ſame time, fit for this purpoſe, but all danger be removed of there being a ſtoppage of building, for want of a ſupply of timber, at any future period; an event extremely probable to take place, from the increaſing difficulty of getting the large kind uſed at preſent in the Royal Dock Yards.

In price of timber;

uſe of ſhorter pieces, &c.;

2d. Much ſhorter timber may, in forming the futtocks, be uſed, without any danger of weakening the ſhip, on account of the great ſupport given to them by the fore and aft ribs, and other internal framing, before deſcribed.

The advantage of this is, that it renders the compaſs timber for futtocks eaſier to be procured, and prevents any neceſſity of uſing any timber cut acroſs the grain.

knees ſuperſeded;

3d. The uſe of knees of every kind is ſuperſeded by this mode of building, as the triangular framing of the decks gives all the effect of lodging-knees, and that of the tranſverſe frames more than ſupplies the ſupport given by hanging-knees.

This would occaſion a conſiderable ſaving in large veſſels, on account of the great price of knee-timber fit for them; which, for that of 30 feet meeting, is near ten pound per load and for the ſmalleſt kind, taken at the dock-yard, not leſs than 8*l.* 15*s.*

thinner planking;

4th. Plank of half the uſual thickneſs may be uſed for lining; the great ſupport given by the fore and aft ribs rendering any uſe of inſide plank, to ſtrengthen the veſſel, needleſs, and confining its purpoſe merely to prevent ballaſt, or other matters, from getting between the timbers, ſo as to reſt on the outſide plank.

This will alſo cauſe a ſaving of conſequence in large veſſels; plank of all kinds, but particularly that of great thickneſs, being the next deareſt article to knee-timber.

leſs quantity of materials;

5th. It is probable a much leſs quantity of timber might be uſed with ſafety in this method, on account of the great ſtrength produced for the timber uſed. 1ſt. By the triangular framing. 2d. By every timber having a ſolid ſupport at each extremity. 3d. By the increaſe of thickneſs from in to out all along the fore and aft ribs, being very great in proportion to the timber uſed.

durability.

6th. It is probable, veſſels built in this method will laſt many years longer before decay; becauſe the uſe of ſmall timber admits of a kind more ſpiny and durable than the large, which is often dotard, and never laſts ſo long; and alſo becauſe this conſtruction admits of a free circulation of air among the timbers, than which nothing is known to contribute ſo much to their preſervation. It is moreover conceived, that the timbers being prevented from working by the ſolid ſupport each has at its extremities, will cauſe the veſſel to wear leſs, and at the ſame time render it ſafer, by diminiſhing the danger of ſtarting planks, or otherwiſe cauſing bad leaks.

7th. The timber of conſiderable ſize uſed in this method is almoſt all nearly ſtraight, or of very little curvature, on account of its running fore and aft.

This kind is much eaſier to procure than large compaſs timber.

8th. Short-top timber and coarſe butts can be worked up to advantage, inſtead of being ſold for leſs than half coſt, or burned; as this kind will do ſufficiently well for the number of ſhort ledges in the deck frames, and to ſupport the lining at the floor, which are wanted in this mode of building,

9th. Veſſels built in this manner will not be ſo liable as others to hog, or have their backs broken, on account of the great ſtrength length-ways, cauſed by the fore and aft ribs.

10th. Veſſels ſo built will be drier, from the circulation of air before mentioned, and having the floor-lining detached from the timbers; which quality renders this conſtruction particularly valuable for the ſhips uſed in the Eaſt and Weſt India trade.

The advantages above recited relate to veſſels entirely formed in this manner. It ſhould be known alſo, that parts of this plan may be applied with profit. The mode of framing the decks, for inſtance, might be uſed to ſave lodging-knees in veſſels built in other reſpects in the uſual mode. Other parts of it might be applied to the ſtrengthening old veſſels, which, by this means, might be made to laſt many years, after they would otherwiſe have been unſerviceable.

The principles of this method of building are capable of being extended ſtill further than they are in the veſſel here deſcribed: the triangular framing may be even adopted to the conſtruction of fore and aft ribs, ſo that theſe could alſo be conſtructed of ſmall timber, if required. Thus, by this means, the former barrier to the increaſe of ſize in ſhips is removed, as it no longer now depends on the ſize of timber; and ſhips of any dimenſions required may be formed, of any ſtrength requiſite, of ſmall timber.

J. W. BOSWELL.

VII.

Concluding Remarks on the Computation of Tables of Squares and Cubes. In a Letter from E. O.

To Mr. NICHOLSON.

SIR,

On the computation of ſquares and cubes.

I AM ſorry to trouble you again on a ſubject which I fear can afford but little intereſt to the generality of your readers; but I muſt requeſt the admiſſion of a few remarks upon the letter which you publiſhed in your laſt number from H. G.

Your correſpondent is certainly miſtaken when he aſſerts that our methods of calculating ſquares are "preciſely the ſame:" Both, indeed, are derived from the principles of the binomial theorem; but I conceive that there is a material difference in our manners of applying thoſe principles to practice. As we find the ſquare of the given number by adding a certain quantity to the known ſquare of the number next below it, it follows, that in both methods the quantity ſo added muſt be the ſame; but there is a conſiderable difference in the operations by which we find this number: I do it by the repeated addition of the ſmall number 2, whereas H. G. finds the ſame quantity by the repeated doubling of a number, which is conſtantly changing, and may poſſibly conſiſt of many figures. To ſhew the difference more clearly, I will calculate ſeveral ſquares according to the direction given in P. 150, Vol. VIII. and I will take for my example the ſame which I have taken in P. 6, Vol. IX. that your readers may the more eaſily form the compariſon for themſelves.

28261		28262	
2		2	
56522	= 2.28261	56524	= 2.28262
798684121	= $\overline{28261}\rceil^2$	798740644	= $\overline{28262}\rceil^2$
1		1	
798740644	= $\overline{28262}\rceil^2$	798797169	= $\overline{28263}\rceil^2$

On the computation of squares and cubes.

$$\begin{array}{rcl}
28263 & & \\
2 & & \\
\hline
56526 & = & 2.28263 \\
798797169 & = & \overline{28263}|^{2} \\
1 & & \\
\hline
798853696 & = & \overline{28264)}^{2}
\end{array}
\qquad
\begin{array}{rcl}
28264 & & \\
2 & & \\
\hline
56528 & = & 2.28264 \\
798853696 & = & \overline{28264}|^{2} \\
1 & & \\
\hline
798910225 & = & \overline{28265)}^{2}
\end{array}$$

Perhaps it may be objected to my ſtatement, that the calculator would be aware that $28264.2 = 28263.2 + 2 = 28262.2 + 4$, &c. or that he muſt ſoon diſcover this law of continuation; and therefore, that he would not proceed in every inſtance to the actual multiplication of the root by 2. Such an objection, however, will avail nothing againſt my argument, for in that caſe he will no longer work by the rule given by H. G. in P. 150, Vol. VIII.; but if he proceeds to find the difference by addition, he muſt virtually purſue the method which I recommend: the only difference will be, that the practice of calculation will have ſuggeſted to him an improvement which he might have originally derived from firſt principles.

I ſhall not enter into the queſtion, whether addition is, or is not, a ſimpler proceſs than ſubtraction. The preſent queſtion may be decided independently of it. For granting that there is no reaſon to prefer one to the other, yet the two methods by which we find the firſt differences of the cubes, might be fairly put to iſſue upon the number of figures uſed in each. To find them according to the method recommended by H. G. in p. 150, Vol. VIII. we muſt ſubtract the whole of one cube from the whole of the other; whereas I find them by the conſtant addition of the ſecond differences, which muſt neceſſarily be much ſmaller numbers. Thus to find the firſt difference between the cubes of 26561 and 26560:

According to H. G.	*According to E. O.*
18738432796481	2116221121
− 18736316416000	+ 159360
2116380481	2116380481

It is ſtated, indeed, by H. G. that he avoids the continual repetition of firſt differences. By this we muſt not underſtand that

On the computation of ſquares and cubes.

that his method makes it unneceſſary to calculate them; for by mere compariſon of p. 9 with p. 125, it will be ſeen that we both uſe the ſame numbers. In this inſtance, as in that of the ſquares, the real difference between the two methods conſiſts in the manner of finding theſe additional quantities. H. G. muſt therefore mean, that he avoids the neceſſity of calculating the table given in p. 8. Now in this I do not think that any advantage is gained. For if the firſt differences are to be found ſeparately, I have ſhewn that they can be more eaſily found by the method which I have recommended; and much expedition will be obtained by making a ſeparate table of them, which is impoſſible if we work according to the directions of H. G. I may, with ſome reaſon, complain, that H. G. has not made his example a fair parallel to mine; for I have ſet down every figure which need be uſed, while he has declined printing any of the operations whereby he determined his firſt differences. It is poſſible, indeed, from long practice in arithmetical computations, that he may be able to caſt his eye from one cube to the other, and ſo find the firſt differences, without copying them out on the ſide of his paper for ſubtraction. I ſhall not dwell upon the danger of miſtake in ſuch a proceſs: the quickeſt and moſt accurate eye could hardly avoid frequent errors, and the common arithmetician will find it attended by conſiderable difficulty. But granting that all this difficulty could be completely conquered, ſtill it would give no ſuperiority to the method recommended by H. G.; for it might be applied to mine with equal, if not greater eaſe and advantage. Thus, in the example given in p. 125; if F were computed by the addition of C and D, inſtead of the ſubtraction of A from E; if we conſider $I = F + G$ inſtead of $= H - E$, $M = I + K$ inſtead of $= L - H$, &c. we ſhould have the differences computed according to my rules. We may remark likewiſe, in this ſhorter method of calculating, that, according to the arrangement given by H. G. himſelf in p. 129, the eye muſt neceſſarily paſs over two intermediate lines of figures in ſubtracting one cube from the other, whereas the two rows of figures which I would have added to one another, may always be placed contiguous. This certainly is an advantage; but at the ſame time I only mention it to ſhew the

grounds

On the computation of ſquares and cubes.

grounds on which I prefer my method to that of H. G.; for I ſtill conſider the plan which I firſt laid down as the eaſieſt and ſureſt. . .

I have ſaid, that if the differences are conſidered in p. 125 as found by addition inſtead of ſubtraction, the example would ſuit with my method, becauſe the ſecond differences are ſtill additional, and will be found, upon compariſon, to be added in exactly the ſame order as in pp. 8 and 9. The only variety is by making two tables: I add the ſum of the two numbers which are ſeparately arranged under each cube. I have only to add on this part of the ſubject, that H. G. has adopted my method in ſome meaſure; for he ſays that $K = G + 6$, as well as $= 6d$. It is true (as I remarked with reſpect to the ſquares, that this equality would moſt probably occur to the calculator; but at the ſame time we muſt recollect, that it is different from the rule laid down in p. 150, Vol. VIII.

I ſhall not add any thing to what I have ſtated in p. 5, with reſpect to the miſtakes to which either method is liable; but I ſhall conclude by aſſuring you, that it is not my intention to trouble you any more on the preſent ſubject. I thought it might be uſeful to publiſh the letter which I firſt ſent you, and having done ſo, it became neceſſary to remove any obſcurities in it, and to anſwer any miſtaken objections which might be raiſed againſt it: but this having once been done, there can be no occaſion for further controverſy; and I have only to thank you for the indulgence which you have granted me.

I am, Sir,
Your obliged humble ſervant,
E. O.

P. S. I take this opportunity to beg you will mention two or three preſs errors which occur in my former letter.

P. 9, line 20, dele the Italic *e*.
P. 10, line 12, $\overline{x + 1}|^3$, read in both inſtances, $\overline{x + 1}|^2$
P. 12, line 16, 29 read 27.

VIII.

Letter from C. Wilkinson, *Eſq. on Galvaniſm and Electricity.*

Dublin, Oct. 8, 1804.

To Mr. NICHOLSON.

SIR,

Obſervations on galvaniſm, &c.

WHEN at Liverpool a few days ſince, at the Athenæum I found a paper of Mr. Thickneſſe of Wigan in your valuable Journal, and which has induced me to trouble you with the following obſervation. Mr. T. ingeniouſly conjectures that the galvanic phenomena depend more upon the decompoſition of the water employed than as to any chemical change effected on the metals. Mr. T. obſerves that two metals are requiſite to the production of galvanic phenomena: this is no ways the caſe, it is well known that a ſingle metal ſuffices, or even brain and muſcle, according to the experiment of La Grave, or nerve and muſcle according to Aldini. Mr. T. further obſerves that hydrogen mixed with copper always renders it brittle. I ſhould wiſh to know this gentleman's authority for ſuch an aſſertion: he alſo ſets out too hypothetically as to electricity being a modification of caloric. It has always appeared to me that the galvanic phenomena entirely depend upon the diſengagement of electricity from the metal undergoing a chemical change.

Galvani has aſcertained that gold, ſilver, copper, iron, tin, lead and zinc, conſtitute the ſeries of metallic bodies; that when two metals the moſt remote in the ſeries are united, the moſt powerful galvanic combination is formed; thus gold and zinc, ſilver and zinc, copper and zinc, &c. and laſtly, lead and zinc, form the weakeſt galvanic combination. The diſpoſition to oxidation is in the inverſe order, thus zinc will become oxidated even by expoſure to the air, whilſt ſilver and gold undergo this change with the greateſt difficulty.

Hence a metal which is oxidated with the greateſt difficulty combined with a metal which oxidates with the greateſt facility, form the moſt powerful galvanic combination. The production of galvanic phenomena is always proportionate to the degree of oxidation.

All

Obſervations on galvaniſm, &c.

All ſubſtances which are conductors of electricity, if they undergo any change as to their conducting power, ſo as to become diminiſhed as conductors, always in the change loſe a portion of their natural electricity. Thus metals which are the beſt conductors, when oxidated, become non-conductors in this change, the combined electricity is loſt.

If a plate of zinc is univerſally immerſed in a fluid which will produce chemical changes upon it, no galvanic phenomenon will be produced becauſe the metal in every aſſignable point, being equally acted on, the contrary ſtates of electricity cannot then be produced.

If only one ſide be acted on, every aſſignable point on that ſide undergoing a chemical change, by which it is diſpoſed to part with its combined electricity, there will be a general tendency of the electricity in the ſubſtance of the metal towards the ſurface acted on; the ſame as if in a veſſel filled with water, a ſmall aperture be formed, there will be a general tendency or current of the water towards the aperture, ſo of electricity.

Dr. Wollaſton has proved by very ingenious experiments, that when two diſſimilar metals are placed in a fluid which will act upon one of the metals and not upon the other, when the two metals are put into contact in the ſame fluid, that thus chemical changes are effected on both; thus that gold and ſilver thus arranged, will be acted on by the nitric acid.—When two metals are placed in a fluid which will act upon either of them ſeparately, if the two metals are preſerved in a ſtate of ſeparation while in the ſame fluid, chemical changes will only be effected upon one. Thus zinc and copper placed in nitrous acid and water, only the zinc will be acted on.

Theſe principles point out the neceſſity in our galvanic troughs to have our cells perfectly inſulated, and that there be no communication between the reſpective plates.

Mr. T. has by miſtake obſerved that the ſenſation is in proportion to the ſurface acted on; the experiments of the French philoſophers proved that the action of galvaniſm on animal ſubſtances is in the ratio of the number of plates employed, and not the ſurfaces expoſed.

In all my experiments in town with my own electric battery I never ſucceeded in giving the ſlighteſt charge with my galvanic apparatus. In ſome converſation I had with you, Mr. Editor, you conjectured that an extenſive ſeries of ſmall jars would be the beſt arrangement. When at Liverpool, I met with ſuch at the houſe of Mr. Dalton, a very ingenious lecturer on electricity. The galvanic battery I employed conſiſted of 200 eight-inch plates, which had fuſed at the time near five feet of ſteel wire; this communicated a very ſlight intenſity to the electric battery, containing forty feet coated ſurface, juſt ſufficient to convulſe a frog, and although the electrical battery was in the beſt ſtate of preparation, we could not ſucceed in producing any intenſity as even to affect the tongue. At another period, I ſhall do myſelf the pleaſure of tranſmitting to you an account of ſome ingenious experiments of Mr. Dalton, on electricity, he finds a cylinder, when exhauſted of air, or when one atmoſphere is condenſed into it, will not then be capable of excitation.

I am, Sir,
Your's ſincerely,
C. WILKINSON.

No. 19, *Soho-Square.*

IX.

Method of preventing Accidents to Horſes and Carriages, in going down Hills, by a Gripe or Claſp acting on the Naves of the Wheels of the Carriage. By Mr. W. Bowler.*

SIR,

Apparatus to lock the wheels of carriages.

THE invention I have now ſent you is, I think, likely to be of great uſe, and I therefore offer a model thereof for your inſpection. In the firſt caſe, this cart may be ſtopped in an inſtant, in going down the ſteepeſt hill with a load, without ſtopping the horſes, by the carter only preſſing his hand upon a lever. This plan would likewiſe be highly advantageous in caſe of a horſe taking fright, as the carriage may be inſtantly ſtopped by the brace claſping the wheels. It may alſo, with little alteration, anſwer for a broad-wheel waggon with a heavy load. By the preſent method, a coachman, whilſt ſit-

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* Tranſactions of the Society of Arts. A bounty of ten guineas was given to the inventor, and the Society have a model.

Apparatus to lock the wheels of carriages.

ting on his box, may confine or releaſe the wheels of his carriage at pleaſure, and prevent accidents in deſcending hills, or in managing reſtive horſes.

I am, Sir,
Your humble Servant,
WILLIAM BOWLER.

Finſbury-Street, May 13, 1803.

To Mr. CHARLES TAYLOR, *Sec.*

Reference to the Engraving of Mr. William Bowler's Gripe for the Wheels of Carts and Carriages, Plate X.

A B. The extent of the body of the cart. C D. The two ſhafts. E. The front ladder of the cart, part of which projects over the ſhafts, and is ſupported on them: the other part is faſtened to the upright piece F. G. The handle of the lever, ſliding betwixt two uprights, and moveable on an iron bar and pivot at H, in the upright piece F. By preſſing down this handle, the moveable joints I K raiſe upwards the lower part of another lever L, ſo as to preſs againſt the lower part of the nave of the wheel, and at the ſame time the moveable joints preſs down the lever O, and cauſe this lever to act againſt the upper part of the nave of the wheel; thus compreſſing the nave of the wheel betwixt a double brace, and either retarding wholly the motion of the wheel, or allowing it to move a little, as may be thought requiſite. The circle of the nearer wheel is ſhown by the dotted lines P; but this wheel is not added, as it would prevent this gripe from being clearly ſeen. The handle of the lever G produces at the ſame time a ſimilar effect on the further wheel R, by means of the bar of iron which croſſes the cart at H, and acts in the ſame manner on ſimilar double joints at the other ſide of the cart, as may be ſeen at S, where the further double gripe is ſhown preſſing on the nave of the further wheel. T. A ſmall catch, which, by falling into the notches or teeth of the lever, when it is preſſed down, holds both the gripes in one certain poſition.

When the wheels are to be ſet at liberty, the catch T is removed out of the teeth of the lever; then the lever G is raiſed above the hole V, and kept from ſliding down by an iron pin attached to the chain U, being put underneath the lever, through the hole V.

The iron-work is not any inconvenience in the loading or unloading of the cart, nor occupies any room within it; and, what is of material conſequence in new inventions, it may be managed by the carter with much leſs time and trouble than the common hook and drag chain. It is alſo much more ſecure in its action, as it binds on both wheels at once, and gives a uniform ſteady draught for the horſes, which prevents them from falling down.

X.

Obſervations and Experiments to elucidate the Operation of the Galvanic Power. By Mr. CHARLES SYLVESTER. *In a Letter from the Author.*

To the EDITOR *of the* PHILOSOPHIC JOURNAL.

SIR,

I HAVE for ſome time read and admired your valuable publication, but have not before this time ventured a communication on any ſubject. If you think the following thoughts and experiments worthy the attention of your readers, I ſhall feel myſelf honoured if you give them a place in your Journal.

I remain, Sir,
Your obedient ſervant,
CHARLES SYLVESTER.

Sheffield, Oct. 16, 1804.

The decompoſition of water, or at leaſt, the preſence of oxigen and hidrogen, is obſerved in all the modes by which the galvanic energy is excited. I obſerve that ſome of your correſpondents are inclined to doubt the truth of water being a compound body, from obſerving in its decompoſition that the oxigen and hidrogen are given out at ſo great a diſtance from each other. Though theſe philoſophers have more ſimply accounted for ſome of the phenomena by making water a ſimple ſubſtance, their aſſumed data, that poſitive and negative electricity are two diſtinct bodies, is by far more gratuitous than the poſition, that water is a compound. Beſides this hypotheſis will only account for a few of the facts, while it abounds with contradiction when applied to the reſt of the phenomena of galvaniſm.

Whether water be decompoſed in the galvanic proceſs;

—or poſ. and neg. electricity be diſtinct bodies.

They tell us of two kinds of electricity coming from the galvanic apparatus one from the zinc end, the other from the copper end: the former being poſitive, the latter negative.

The latter leſs probable.

Two diſtinct fluids moving in contrary directions could not poſſibly exiſt in the galvanic trough; by reaſon of that fluid proceeding from the zinc meeting that coming from the copper in each of the cells, and conſequently eſtabliſhing a conſtant equilibrium, without producing any effect. When the decompoſition of water is only taking place in one veſſel (admitting the above objection to be of no weight) the phenomena admit of an eaſy explanation by this hypotheſis; but when two veſſels are uſed connected by a wire arch, it cannot in any ſhape be adequate; for as we have in each veſſel both oxigen and hidrogen gaſes, there muſt alſo be both poſitive and negative electricity; theſe two contrary fluids would of courſe have to meet each other in the connecting arch, and according to another part of this theory form ſenſible heat *.

The oxidation contradicts the hypotheſis of two electric bodies.

The poſitive wire, if a baſe metal is always oxidated; this effect is known to be facilitated by the electrical agency. According to the principles in queſtion, it would be retarded by the power of affinity exiſting between water and poſitive electricity. When the wires of a galvanic apparatus are brought into a metallic ſolution, no hidrogen is given out at the negative wire, but the metallic oxide is reduced. Now if metallic oxides be compoſed of water and the metal, the negative electricity would combine with the water, and hidrogen gas would be evolved—the very reverſe of the fact.

My reaſon for being thus particular in the examination of the hypotheſis of Mr. Richter and Dr. Gibbs, is to prepare the mind of the reader for ſome experiments which I think will contribute to the firmneſs of the hypotheſis, which ſuppoſes water to be a compound, and that the hydrogen is carried from the poſitive to the negative wire by the electricity.

Variation of Dr. Aſh's experiment.
When a long copper wire was connected at the end with zinc (under weak ſulph. acid) hidrogen was earlier given out

The well-known experiment of Dr. Aſh (with the plate of zinc and ſilver in a dilute ſulphuric acid) I had occaſion to vary in the following manner. I took a copper wire about eighteen inches long, and bent it in the middle ſo as to form

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* They are of opinion that theſe two fluids combined form caloric. They alſo hold with Dr. Prieſtly that what are now termed metallic oxides are the reſpective metals combined with water.

two parallel legs, about two inches diſtant from each other. This I laid in an earthen diſh filled with dilute muriatic acid. I then brought a piece of zinc in contact with one of the ends: bubbles of hidrogen were immediately given out by that part of the copper. After bubbles had appeared for about two inches down this leg, I obſerved bubbles on the end of the other leg,—they now proceeded down the two legs till at laſt they appeared on the part where the wire was bended. I afterwards laid a piece of zinc at one end of the ſame diſh and a piece of gold at the other. I prepared a metallic arch with two pieces of wire, one of gold, the other zinc, ſoldered together. The zinc end I connected with the piece of zinc in the diſh, and the gold end with the gold. After the contact had been made about ten ſeconds, bubbles appeared on the piece of gold. The diſtance was about twelve inches. I now placed the pieces of gold and zinc at half the diſtance, and made the communication with the ſame arch. The bubbles now appeared upon the gold in about half the before-mentioned time. Upon bringing the pieces ſtill nearer together, the gold gave out gas almoſt inſtantly, but yet in all the three inſtances I had time to obſerve that the bubbles always appeared firſt on that ſide the gold next the zinc.

at the nearer parts of the (bended) wire;

—and ſo likewiſe in a compound conductor between gold and zinc.

I think from the above experiments that the diſengagement of hidrogen from the gold does not depend upon its negative ſtate, for this quality muſt have been produced on the gold in every one of the three inſtances in the ſame time, from the communication being always made by an arch of the ſame length. On the contrary, it proves that the electricity diſengaged from the zinc is communicated to the gold through the intervening liquid. If we obſerve the length of time taken up in its paſſage through this medium, we ſhall ſee that it does not agree with the laws of electricity. Is it not therefore probable that when the oxigen of the water combines with the metal, the hidrogen combines with the electricity of the metal, and a compound of this kind obſerves thoſe laws conſiſtent with the phenomena.

Whence it is inferred that the diſengagement of hidrogen does not depend on a neg. ſtate; but that the electricity is communicated through the interpoſed liquid.

In each of the cells of the galvanic trough a quantity of this compound is diffuſed through the liquid. The moment a communication is made between the two ends, the electricity enters each of the copper-plates, leaving the hidrogen in bubbles

Probability that the hidrogen is in combination with electricity.

on the ſurface *. As this electrated hidrogen (if I may be allowed the expreſſion) does not move through the liquid with the facility of the electricity itſelf, the neceſſity of a ſurface of copper equal to that of the zinc is obvious. It is plain that only a proportionate quantity will be carried through the whole ſeries. This fact I have aſcertained in attempting to render the apparatus propoſed by Mr. Wilkinſon and yourſelf uſeful for all experiments. The ſhock is even leſs when the ſurface of copper is leſs than a ſquare inch.

Objection to the apparatus deſcribed in our Vol. VIII. p. 3. on the couronne of Volta.

How the electricity increaſes the hidrogen in the exterior cells.

The quantity of electrated hidrogen in each cell increaſes from the copper end to that of the zinc in an arithmetical progreſſion. The electricity only exiſts in its ſimple form while it is paſſing from the copper ſurface through the two metals to the zinc ſurface. It there combines with another portion of hidrogen, which it leaves at the next copper ſurface, and ſo on accumulating in quantity the longer the ſeries it has to paſs through. In the decompoſition of water by the galvanic trough the veſſels in which this is performed may be conſidered as one cell in the ſeries. The electricity ſeizing the hidrogen at the poſitive wire and giving it out at the negative.

XI.

Memoir on the Origin of Wax †. *By* Francois Huber, *Member of the Society of Natural Philoſophy and Natural Hiſtory of Geneva.*

It has been thought ſtrange that the word wax ſhould ſeldom occur in a book which treats of bees alone: but nevertheleſs as in the courſe of my obſervations, I had not attended to the products of their induſtry, I could only have repeated what had been ſaid by Swammardam and Reaumur, and that did not ſeem to me to be neceſſary.

Mechanical operations of the bees.

I knew that theſe inſects collected abundantly upon the antheræ of flowers, that they are acquainted with the method of opening them, of gathering their duſt, keeping it in the cavities of their hind legs, and carrying it to their hives.

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* This fact is very obvious when the copper ſurface is very clean. The hidrogen under contrary circumſtances is employed to reduce the oxide.

† From Journal de Phyſique, &c. Pluvioſe, An. XII.

Examination o
the pollen of
flowers:

It had been obſerved that the particles of this duſt ſwells in water, and that, when one of them burſts, an oily liquor runs out, which floats on its ſurface, but did not mix with it; from theſe experiments, repeated on the duſt of a great number of flowers, it was concluded that they contain the principles of wax; but it was admitted that theſe muſt undergo a peculiar elaboration in the body of the bee, ſince, according to the experiments of Reaumur, a flexible wax could not be made from the duſt of the antheræ.

The true origi
of wax remained ſo lon
unknown fro
an opinion th
it was diſcove

It will be ſeen from ſeveral paſſages in my work that I had adopted this opinion: a ſingle obſervation of Burnens* changed all my ideas. The true origin of wax might have been ſooner known, had there been any ſuſpicion that it was not already diſcovered. I ſhall now ſtate how I was led to doubt, and what I have done to verify my new conjectures.

Obſervations
which led to
the enquiry.

I was in Switzerland in 1793; the farmer of the eſtate on which I reſided had many bees, and the greater part of his hives having been ſtocked in former years, the combs with which they were filled reached to the ſtands, conſequently there was no room to conſtruct new ones. We remarked, however, that the working bees carried in a conſiderable quantity of this fecundating powder.

There was alſo in the ſame apiary ſome ſwarms of that year, the hives having only been ſtocked a day or two; in ſome of them the combs were only began, in others they were larger, but in all of them there were vacancies to fill up, and much work to do. We obſerved with aſtoniſhment that the bees of theſe ſwarms did not carry in the pollen, and that, nevertheleſs, they worked with activity in the conſtruction of new combs, and in lengthening thoſe already commenced. Where, therefore, did they procure materials for their edifices? After theſe obſervations, we ſuſpected that it was not from the duſt of the ſtamina, and that they had a very different uſe for it than that for which it was believed to be intended. We however found that it was not impoſſible to explain theſe extraordinary facts, without abandoning the hypotheſis of Reaumur,

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* The name of M. Huber's ſecretary; which deſerves to be known to the cultivators of natural hiſtory.

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mour, by ſuppoſing that the bees of the old hives ſtored up ſo much pollen in their combs for their future wants, while thoſe of the new ſwarms did not carry it outwardly on their legs, in the infancy of their eſtabliſhment, becauſe they had no cells in which they could depoſit it: it might be ſufficient to enable them to conſtruct their combs, if they were at liberty to fly to the flowers, procure their pollen, and return to their hives after having filled their ſtomachs, where it muſt be elaborated and converted into perfect wax. It was to obviate theſe doubts that I undertook the following experiments.

FIRST EXPERIMENT.

On Bees in Confinement with Honey alone for their Nouriſhment.

Exp. I. Is pollen neceſſary to the production of wax?

Muſt bees eat pollen to be in a ſtate to produce wax? This was the firſt queſtion which I thought it neceſſary to inveſtigate; the method of trying the experiment was obvious; it was only required to keep the bees within their hives, and thus prevent them from collecting or eating the fecundating powder.

On the 24th of May, Burnens lodged a ſwarm in a ſtraw hive, with as much honey and water as was neceſſary for their conſumption, and he cloſed the doors ſo that the bees could not get out and the air be at the ſame time renewed.

Wax from honey alone.

At firſt the bees were very uneaſy, but became calm on removing the hive to a cool dark place: their captivity laſted five days; they were permitted to come out in a room, the windows of which were ſhut: we then examined the hive more conveniently. We firſt noticed that there was no honey left in the veſſel which had been filled with it, with the ſole intention of feeding the confined bees; and were more aſtoniſhed to find five combs of the moſt beautiful wax, ſuſpended from the roof of the hive; they were perfectly white, and very brittle. This reſult was very remarkable; however, before forming a concluſion from it, that the honey with which theſe bees were fed had enabled them to produce the wax, it was neceſſary to enquire whether it could not alſo be explained in another manner.

The bees which I had employed had doubtleſs collected the duſt while they were at liberty.

They might have done ſo the evening before, or on the very day of their confinement, and might have enough in their ſtomachs, and in the cavities of their legs, to extract from it all the wax which we had found in their hive.

But if it was true that it had been obtained from the fecundating powder previouſly obtained, this ſource was not inexhauſtible, and the bees being unable to procure any more, they would ſoon ceaſe to conſtruct combs, and fall into the moſt complete inaction. It was neceſſary therefore to continue the ſame trial to render it deciſive.

The 28th, Burnens returned this ſwarm into its hive, after having taken out all the combs; he ſhut them up as before with a freſh ſupply of honey.

This experiment was not long, for on the evening of the ſecond day we perceived the priſoners working with new wax. The next day the hive was inſpected, and we found five combs, as heavy and as regular as thoſe made during their firſt captivity.

We afterwards repeated this experiment five times ſucceſſively, with the ſame bees, and the ſame precautions: we always found that the honey had diſappeared, and that new wax was produced. This reſult was ſo invariable during this long ſecluſion that we could no longer doubt that the honey alone had ſupplied them with all the elements of their wax, without the aſſiſtance of the fecundating duſt.

SECOND EXPERIMENT.

On a Hive from which Honey was excluded, and in which only Pollen and Fruits for the Nouriſhment of the Bees were left.

I thought it would not be uſeleſs to make the inverſe of the preceding experiments: it would ſhow me whether the pollen could not ſupply the want of honey when the bees were deprived of it, and enable them to produce wax. Exp. II.

I therefore encloſed a ſwarm in a bell-glaſs, in which had been placed a comb whoſe cells contained only pollen, and the ſole nouriſhment of the bees was fruit.

Theſe bees did not make wax, nor did they form a ſingle cell during eight days, which was the time of their captivity.

I was going to repeat this experiment, when Burnens remarked that the free bees were, in ſome meaſure, in the ſame ſtate as thoſe we had confined; there being no honey at that time in the flowers, they found only pollen, and did not work in wax.

Bees fed on pollen and fruits do not produce wax.

I may perhaps be aſked how I was ſatisfied of this, to which I anſwer, bees-wax is white at firſt, the cells ſoon become yellow, and in time, this colour grows browner, and in older hives the combs have acquired a blackiſh tinge. It is therefore very eaſy to diſtinguiſh the new cells from thoſe which have been ſome time formed, and conſequently to know whether the bees are really making combs, or whether that work is ſuſpended; it is ſufficient to raiſe the hives, and to notice the lower edges of the combs.

The odour exhaled by the hives, and the ſhape of the bees, are indications by which it may always be known whether there is honey in the flowers; if they are combined, there can be no further doubt, and, particularly, if a great number of bees return to the hive, which are remarkable for the bulk and the form of their bellies. Thoſe which are filled with honey have the abdomen cylindrical; the name of wax-making bees belongs to them excluſively: the bellies of the labouring bees which have other functions, always preſerve their ovoid form, and their volume is never ſenſibly augmented; the name of nurſing bees is proper for theſe.

Wax-making bees.

Nurſing bees.

The farmers of the neighbouring villages kept their bees in baſkets, or in caſes of different forms; and I was able to viſit a very great number without going to any great diſtance from my habitation.

Operations of the bees during an intemperate ſpring,

In 1793, an intemperate ſpring had retarded the ſeparation of the ſwarms; there had not been any in the country before the 24th of May; but towards the middle of June there were ſeveral in the vicinity of my reſidence. At that time the fields were covered with flowers, the bees collected much honey, and the new ſwarms worked at the wax with vigour.

On the 18th, Burnens viſited ſixty-five hives; at the entrances of all of them he obſerved wax-making bees. Thoſe which returned to old hives, not having to conſtruct cells, depoſited their honey in the combs, or diſtributed it among their companions; thoſe belonging to the ſwarms converted their honey into wax, and haſtened to conſtruct combs for the reception of their young bees.

It was ſhowery on the 19th: the bees went abroad but brought home only pollen. The weather was cold and rainy until the 27th. We were deſirous of knowing if this had prevented their working. On the 28th, all the hives were lifted:

Burnens found that the work had been ſtopped; the combs which he had meaſured on the 19th, were not at all increaſed, and were of a citron-yellow, nor was there a ſingle white cell in any of theſe hives.

On the 1ſt of July the cheſnuts and limes were in bloſſom, the thermometer indicated the 20th degree; the wax-making bees re-appeared, they carried away great quantities of honey, which, as we had before obſerved, was employed in augmenting the proviſions of the old hives, and in enabling the young ſwarms to conſtruct new combs.

—and a dry ſummer.

The greateſt activity was obſervable among them: the gathering of honey, and the production of wax continued until the middle of this month.

July 16th, the heat remained the ſame: the field flowers, as well as thoſe of the cheſnuts and limes, were completely withered; they yielded no more honey; their pollen alone attracted the working bees, and they collected it abundantly, but there was not any wax produced: the combs were not lengthened; thoſe of the young ſwarms did not fill more than two thirds of their hives.

Auguſt 9th. It had not rained for ſix weeks, the heat was very powerful, nor was there any dew to allay it during the night: the black wheat which had been in flower for ſome days, did not offer any honey to the bees; they found only pollen.

On the 10th, it rained for ſeveral hours; next day the black wheat had the odour of honey; in fact it might be ſeen glittering in their expanſed flowers. The bees found enough to feed them, but too little to induce them to work at new wax.

On the 14th, the drought re-commenced, and laſted to the end of the month: no more honey appeared upon the flowers, and when we viſited the ſixty-five hives for the laſt time, we found, 1ſt, that the bees had not produced any wax after the middle of July; 2d, that they had ſtored up a great quantity of pollen; 3d, that the ſupply of honey was much leſſened in the old hives, and that hardly any remained in the new ſwarms, that which they had collected in the ſpring having been employed in the preparation of wax; the pollen, therefore, has not this property, and no further doubt remained on this head.

This year had not been ſtormy, and I have ſince aſcertained, by a great number of obſervations, that electricity is ſingularly favourable

Weather moſt favourable to the labour of bees.

favourable to the ſeeretion of honey by the flowers: the bees never collect it in greater abundance, nor is the preparation of wax ever more active than when the wind is in the ſouth, the air humid and warm, and a ſtorm gathering.

Heat too long continued, and the drought which is the conſequence of it, cold rains, and principally a north wind, ſuſpend it entirely.

THIRD EXPERIMENT.

On the Uſe which the Bees make of the fecundating Powder.

Exp. III.

In the ſecond experiment the bees did not touch the pollen which I had placed within their reach, and, as its quantity was not ſenſibly diminiſhed during this trial, I was induced to believe that it was not an aliment proper for them.

What is the uſe of pollen ?

I alſo knew that the new ſwarms were liable to periſh from hunger in the middle of ſummer, and even when the country was covered with flowers, if a particular temperature, which is too uncommon in our climate, did not favour the ſecretion of honey in their nectaria. What, therefore, is the uſe of the pollen which they collect with ſuch avidity during eight months of the year, and of which they lay up ſuch abundance?*

This queſtion remained to be inveſtigated.

I had a hive, in diviſions, the queen of which was barren; its combs did not contain any pollen, but they had much honey: the two narroweſt ſides of this hive were formed of panes of glaſs, through which the ſurfaces of the exterior combs might be ſeen, and the conduct of the bees obſerved.

The queen, bee taken away.

I took away the queen on the 16th of July, but to conſole the working bees I removed the firſt and the twelfth combs, in which there was not any thing to intereſt them, and I ſupplied their places with two combs, the cells of which were filled with eggs, and worms of all ages. I carefully cut away all the cells in which pollen could be perceived, and ſhut up the hive with a grating. My intention will be gueſſed: I wiſhed to know whether theſe inſects could ſupport their young without this fecundating powder.

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* Reaumur was of opinion that the bees of a well-ſtocked hive might collect at leaſt a hundred pounds of this ſubſtance in the courſe of a year; but, having remarked that the weight of wax, fabricated in the ſame time, did not exceed two pounds, he concluded " that the bees extract only a very ſmall portion of the true wax from this native wax, that the greateſt part of it is required for their nouriſhment, and, that the reſt is diſcharged from their bodies in the form of excrement."

The next day nothing extraordinary occurred; the bees ſat on their eggs and ſeemed to nurſe them.

On the 18th, after ſun-ſet, a great noiſe was heard in the hive. Anxious to ſee what occaſioned it, we opened the ſhutters, and obſerved that all was in confuſion: the incubation was ſtopped; the bees ran over the combs in diſorder; we ſaw thouſands precipitate themſelves on the ſtand, thoſe which were neareſt to the mouth eagerly gnawed the grating; their intention was no longer doubtful, they wiſhed to get out of their confinement.

I was fearful of deſtroying them by continuing to prevent them from yielding to their inſtinct, they were therefore ſet at liberty: the whole ſwarm came out, but the hour was unfavourable to their collecting, the bees did not go far from the hives, the darkneſs, and the chillineſs of the air, ſoon compelled them to return, and probably calmed their agitation, for we ſaw them quietly reaſcend their combs, and order appeared to us to be re-eſtabliſhed. This moment was taken to cloſe the hive again.

Other royal cells made,

On the 19th, we ſaw two royal cells begun on one of the combs of the nurſery *(couvain)*; the evening of this day, and at the ſame hour as the day before, we heard a great tumult in the cloſed hive, it was in a general confuſion, and we were again obliged to permit the ſwarm to come out.

The 20th was the fifth day of their captivity; we thought it had been of ſufficient duration, and were alſo very impatient to examine the nurſery, and to ſee what was the cauſe of the periodical agitation of theſe bees: Burnens therefore opened the firſt and twelfth windows, and drove the bees from the combs, ſuffering them to take their flight in a room, the windows of which were ſhut.

He firſt noticed that the royal cells had not been continued, that they did not contain any worm, and that there was not an atom of the jelly which ſerves for the nouriſhment and the cradle of the larvæ of the queens.

He ſought in vain for eggs, for worms, and for the liquid in the common cells; all had diſappeared. Had theſe worms died

—but the proceſs did not go on without pollen.

died of hunger? Had we, by withdrawing the fecundating powder, deprived the bees of every means of nouriſhing them?

To aſcertain this it would be ſufficient to reſtore them their pollen, and obſerve the iſſue. The bees were therefore again returned to their priſon, after having ſubſtituted young worms for thoſe which had been ſuffered to die.

On the 22d, we found that the bees had faſtened theſe combs, and that they were again in a ſtate of incubation; we then gave them ſome pieces of combs in which other bees had ſtored up the fecundating powder, and, the better to obſerve what they did with it, we took ſome of the pollen out of the cells, and laid it expoſed on the ſtand of the hive. In a few minutes the bees diſcovered the pollen in the combs, and that which we had taken out; they took it grain by grain in their jaws, and conveyed it into their mouths; thoſe which had eaten moſt voraciouſly re-aſcended the combs, and placed themſelves, at firſt, upon the cells of the young worms, which they entered with their heads foremoſt, and remained there a greater or leſs length of time. One of the windows of the hive was now opened cautiouſly, Burnens powdered the bees which eat the pollen, and watched them for ſome hours; he obſerved that the marked bees always re-aſcended to the nurſery, and immediately entered the cells of the young bees.

The 23d, we found the royal cells begun.

The 24th, we drove the bees from off the young worms, and we remarked,

1ſt, That all of them had the jelly, as in the common hives:

2d, That the worms had grown larger, and were forwarder in their cells:

3d, That others had been ſhut up again: And,

4th, That the royal cells had been lengthened.

The 25th, we withdrew the pieces of comb which we had placed on the ſtand, and found that the quantity of pollen was ſenſibly diminiſhed; we afterwards replaced them in the hive with other cells filled with the fecundating powder.

The 26th, the royal cells had been cloſed during the night, as well as ſeveral of the common ones.

The 27th, I reſtored theſe bees to liberty; Burnens examined the cells with the greateſt attention, and found jelly in all thoſe which ſtill contained worms, but moſt of them were ſhut with a lid of wax: he examined ſome of the latter, and found the worms employed in ſpinning cocoons of ſilk.

All the worms had therefore been tended as in the natural hives. In this ſecond trial we did not perceive any diſorder in this hive; there had not been the leaſt agitation: it is true ſome of the working bees attempted to go out in the courſe of the day, but finding it impoſſible, they re-aſcended the combs quietly, which were never left for an inſtant. The hive being abundantly ſupplied with honey, and with the pollen neceſſary for their young, left them nothing to wiſh for; and they were ſtill more happy when a queen was born, who afterwards became pregnant, and laid a great number of eggs.

After theſe two experiments there could be no more doubt that the fecundating duſt was the aliment proper for the young bees, and that the want of this ſubſtance was the cauſe of their death, and of the evident anguiſh of their nurſes during their firſt captivity.

The pollen is therefore the food of young bees.

FOURTH EXPERIMENT.

On Bees deprived of Honey and Pollen, and which it was attempted to feed with Sugar.

I wiſhed to know if it was the ſaccharine part of the honey which enabled the bees to produce wax.

Exp. IV. What part of the honey contains the principles of the wax?

Burnens confined a ſwarm in a glazed hive: one pound of Canary ſugar was their ſole aliment.

He put a ſecond ſwarm into another hive, and endeavoured to feed them with very coarſe raw ſugar; and to obtain a term of compariſon, a third ſwarm was ſhut up in the ſame manner, and fed with honey.

The bees of the three hives produced wax; thoſe fed with the different qualities of ſugar produced it ſooner than the ſwarm which had only had honey, and they produced it in greater quantity.

A pound of Canary ſugar reduced to ſyrup, and clarified with white of egg, yielded 10 gros, 52 grains, of a wax not ſo white as that which the bees extract from honey.

Wax made from true ſugars of different qualities.

An equal weight of raw ſugar gave 22 gros of very white wax.

Maple ſugar produced the ſame effect. This experiment having been repeated ſeven times ſucceſſively, always employing the ſame bees, we could not doubt that ſugar contains the principles of wax, and we concluded that it was the ſaccharine part of the honey which had this property.

CONCLUSION.

Concluſions: Theſe obſervations ſhew,

1ſt, That the wax comes from the honey:

2d, That the honey is alſo a food of the firſt neceſſity to the bees:

3d, That flowers do not always contain honey, as has been imagined; that this ſecretion is ſubject to the variations of the atmoſphere; and, that the days in which it is abundant are very rare in our climate:

4th, That it is the ſaccharine part of the honey which enables the bees to produce wax:

5th, That raw ſugar yields more wax than honey, or refined ſugar:

6th, That the duſt of the ſtamina does not contain the principles of wax:

7th, That this duſt is not the food of the adult bees, and that they do not collect it for themſelves:

8th, That the pollen affords the only aliment which is proper for their young, but that this ſubſtance muſt undergo a peculiar elaboration in the ſtomachs of the bees to be converted into an aliment, which is always appropriated to their ſex, their age, and their wants, ſince the beſt microſcopes do not ſhew the particles of pollen, or their coverings in the liquor prepared by the working bees.

I ſhall ſpeak of the economical conſequences of theſe obſervations on another occaſion. By ſhowing the breeders the real wants of the bees, they will be poſſeſſed of the means of aſſiſting them in time, in all their neceſſities, and of preſerving them in climates in which nature has not placed them, and in which they could not proſper without our aid.

XII.

An Enquiry concerning the Nature of Heat, and the Modes of its Communication. By BENJAMIN *Count of* RUMFORD, *V. P. R. S. &c. Abridged from the Philoſophical Tranſactions for* 1804.

(Concluded from p. 63.)

Exp. 12. Bodies alike in all reſpects and heated, do, at equal diſtances, affect the thermoſcope equally.

Exp. 12. TWO equal cylindrical veſſels of ſheet braſs poliſhed very bright, each three inches in diameter and four inches long, ſuſpended by their oblique necks in a horizontal poſition, being placed on their wooden ſtands, were filled with water at the temperature of 180°; and their circular flat bottoms were preſented, in a vertical poſition, to the two balls of the thermoſcope, at the diſtance of two inches.

When the two hot bodies were preſented, at the ſame moment, to the two balls of the inſtrument, or what was ſtill better, when two ſcreens were placed before the two balls, at the diſtance of about an inch, and, after the hot bodies were placed, theſe ſcreens were both removed at the ſame inſtant, the bubble remained without motion in the middle of the horizontal part of the tube of the inſtrument.

If the diſtances of the equally hot bodies were rendered unequal, the bubble always moved towards the moſt remote of the two; and if a ſingle hot body was preſented to one of the balls, the bubble was driven from it, and might have been carried quite out of the tube; which, however, was always avoided, as the inſtrument would have been by that means quite deranged.

Exp. 13. A cylinder blackened with ſmoke, gives out more radiant heat than a clear metallic ſurface.

Exp. 13. The flat circular bottom of one of the cylindrical veſſels was blackened by holding it over the flame of a wax candle, the other veſſel remaining bright as before. Both were then filled with water at 180°, and preſented at equal diſtances to the two oppoſite balls of the inſtrument, as deſcribed in the laſt experiment.

The bubble was inſtantly driven out of its place by the ſuperior action of the blackened ſurface; and it did not return to its former ſtation until the blackened ſurface had been removed to more than eight inches from the ball to which it was preſented; the other, which had not been blackened, remaining in its firſt poſition, at the diſtance of two inches.

The other coatings which had accelerated the cooling, were alſo found to increaſe the radiation.

Other ſimilar experiments were made to ſhew whether thoſe coatings or clothings which, in the former experiments with the large veſſels, had accelerated their cooling, did alſo increaſe their power of radiation, as ſhewn by the proceſs and inſtruments laſt deſcribed. And the reſults invariably proved that this is the caſe.

It was alſo a queſtion deſerving to be inveſtigated, whether any peculiarity belonging to the metallic ſurface hitherto uſed, namely of braſs, might have influenced the reſults.

Exp. 14, 15. All metallic ſurfaces radiate equally.

Exp. 14, 15. The two large cylindrical veſſels No. 1 and 2, were covered with a ſingle coating of oil varniſh, and on this, when ſufficiently dry, was laid a covering of thick gold leaf upon No. 1, and thick ſilver leaf on No. 2. Theſe veſſels were cooled through the interval of 10 degrees, in the ſame time as the naked veſſel uſed as the ſtandard. Similar experiments with veſſels of tinned iron and of lead, ſhewed that the radiation from all theſe metals, though ſo different in their conducting power, is the ſame.

" Is not this," ſays the Count, " owing to their being all equally wanting in tranſparency? And does not this afford us a ſtrong preſumption that heat is, in all caſes, excited and communicated by means of radiations or undulations, as I ſhould rather chooſe to call them?"

And he proceeds to obſerve, that another very important queſtion alſo muſt be decided before theſe points can be determined, and that is, Whether bodies are cooled in conſequence of the rays they emit, or by thoſe they receive? The celebrated experiment of Pictet has ſhewn, in our author's opinion, that rays or emanations *proceed from cold bodies,* which may be concentrated by concave mirrors, and will affect a delicate air thermometer.

Exp. 16, 17. A cold body depreſſes the temperature of other bodies by radiation. Metal horizontally placed.

Exp. 16, 17. The horizontal cylindrical veſſels (*Fig.* 3, *Plate I.* of the preſent volume) were made very clean and bright, were duly fixed, and left for ſeveral hours in a room near the thermoſcope; and when each veſſel was in ſucceſſion preſented to that inſtrument, with every precaution to prevent irregularity from external circumſtances, it was not found to be affected by them. One of the veſſels was now taken away and filled with ice and water, and then preſented, at the diſtance of four inches, to one of the balls of the thermoſcope. The bubble immediately moved towards the cold body, and paſſed through the ſpace of one inch. A nearer approach of the cold body produced a ſtill farther motion in the ſame direction.

Exp. 18, 19. Though this reſult appeared to the Count to prove indiſputably that cold bodies emit rays capable of generating cold in warmer bodies, yet, from the importance of the fact, he choſe to vary the ſubſtance preſented to the inſtrument, as well as to remove all ſuſpicion of the action of cold currents of air. He therefore laid the thermoſcope on one ſide, and placed underneath one of its balls a ſolid cake of ice, at the diſtance of ſix inches. The reſult of this experiment was the ſame as of the other, and the bubble was moved one inch in the tube. Ice-cold water produced the ſame effect as ice itſelf.

Exp. 18, 19. Ice and ice-cold water underneath the thermoſcope, cooled it by radiation. No current of air could here affect.

Exp. 20. Whether this effect of cold bodies be governed by the ſame laws as thoſe obſerved by varying the nature of the ſurface in heated bodies, or by any other, now remained to be aſcertained. It was before found that metal blackened over a candle, did emit much more of calorific rays than the ſame metal when naked. The ſame experiment was now made with cold bodies. One of the cylinders had its end blackened, and the other cylinder was left bright and naked. Both were filled with ice and ſalt, and at the ſame inſtant they were ſuffered to act from equal diſtances on the thermoſcope. The bubble moved towards the blackened body; not indeed ſo much as when the bodies were heated in the former experiments, becauſe the temperature was not here ſo far diſtant from the common temperature of ſurrounding bodies as in that experiment; but on ſeveral repetitions of the experiment with theſe cold bodies, the effect was conſtantly the ſame. It was found that the precipitation of ice out of the ſurrounding air tended ſpeedily to raiſe the temperature, and alſo that the clean ſurface, when covered with ice, had a greater frigorific effect than when the metal was naked.

Exp. 20. Metal blackened with ſmoke cooled the inſtrument more, by radiation, than a clear ſurface at ſame temperature.

Ice radiates more than metal.

Exp. 21. The radiation of heat from animal ſubſtances appearing, from ſome facts, to be conſiderable, a piece of gold-beater's ſkin was applied wet to one of the veſſels, and remained firmly adherent when dry. This, when filled with hot water at 180°, and preſented to the thermoſcope in oppoſition

Exp. 21. Gold-beater's ſkin radiates 25 times more than metal. Hot water in the veſſels.

to the other veſſel clean and bright, and alſo filled with the ſame fluid at the ſame heat, was found to be much more calorific. The bubble which moved from the coated body, did not return to its ſtation until this body was removed to a diſtance five times as great as that of the other. Whence our author concludes, that it emitted twenty-five times the quantity of calorific rays.

Exp. 22: Similar effect with ice-cold water. The gold-beater's ſkin produced more cold by radiation.

Exp. 22. The veſſels uſed in the laſt experiment were emptied and refilled with ice and water. They were then preſented at equal diſtances from the reſpective balls; and the effect of the body which was covered with gold-beater's ſkin, was much more conſiderable in producing cold.

The radiation of cold bodies appearing to the Count thus to be proved beyond all doubt, he was deſirous of aſcertaining whether the frigorific rays poſſeſs an equal power of affecting the temperature of bodies as the calorific rays do; the temperatures of the radiant bodies being at the ſame diſtance each way from the body to be acted upon.

Exp. 23. A hot and a cold veſſel equally diſtant from the common temperature, affect the thermoſcope equally.

Exp. 23. With this intention, one of the veſſels filled with pounded ice and water, was preſented to a ball of the inſtrument, and the other veſſel filled with water at 112°, was preſented at an equal diſtance on the oppoſite ſide of the ſame ball; the temperature of the room and inſtrument being 72°, or 40° diſtant from each of the temperatures of the veſſels, and the other ball of the inſtrument being defended from all radiation by ſcreens. The bubble remained motionleſs; ſo that the oppoſite actions were in fact equal. And when either veſſel was drawn farther off, the effect of that veſſel became leſs, and the bubble moved; that is to ſay, towards the ball if the cold veſſel were neareſt, or from the ball if the hotter; and theſe effects were equal in quantity as well as in celerity of motion.

Why the cooling by radiation has been leſs noticed.

The Count again repeats his concluſion from theſe experiments lately exhibited to Profeſſor Pictet, M. de Sauſſure, and M. Senebier at Geneva, that the rays which generate cold are juſt as real and juſt as intenſe as thoſe which generate heat; and he proceeds to account for this reſult having been overlooked, by obſerving, that the degrees of cold we are able to produce, are much leſs diſtant from the uſual temperature than thoſe of heat, which are within our power. Thus a cannon ball, heated to 160 degrees, or 70 degrees above blood-heat, would radiate quite as much as ice; and a bullet of freezing mercury would radiate ſcarcely more than another of boiling water: both which hot temperatures are very trifling in compariſon to the heat in which we are habituated to notice and obſerve this phenomenon.

Exp. 24. After the proof that cold bodies of the ſame kind affect the thermoſcope equally, when equally diſtant from the common temperature, it remained to be determined whether the different modifications of ſurface have the ſame effects in the ready propagation of lower temperatures, as they had ſhewn before in higher. To ſhew this, it was only required to oppoſe them to each other, as to their action upon the ſame ball, as in the laſt experiment. With this view both diſcs were blackened, and, the temperature of the room being 72° as before, one of them was charged with ice and water, and the other with water at 112°. Theſe, at equal diſtances from the ball as before, did not affect the bubble, and therefore their actions were preciſely equal.

Exp. 24. The ſame ſurfaces which produce moſt heat by radiation, do alſo produce moſt cold.

In the conſideration of two kinds of rays, calorific and frigorific, it did not eſcape the attention of our author, that hot and cold are terms denoting mere relations; ſo that the ſame body will be either hot or cold accordingly as the common temperature, or temperature of the bodies of compariſon, is lower or higher. Queſtioning, therefore, as to the difference between calorific and frigorific rays, he demands whether the ſame rays may not be either calorific or frigorific, accordingly as the body at whoſe ſurface they arrive, is hotter or colder than that from which they proceed?

Do not the ſame rays produce heat or cold, as the temperature of the emanating bodies is higher or lower than the receiving body.

Exp. 25. The whole of the external ſurface of one of the large cylindrical paſſage thermometers, *Fig.* 1, *Plate I.* was covered with gold-beater's ſkin, and, along with the ſtandard inſtrument of the ſame kind, was filled with hot water. The covered inſtrument cooled through the ſtandard interval of ten degrees, namely, from 101½ to 91½, in twenty-ſeven and three quarter minutes; but the naked inſtrument employed 45 minutes in paſſing through the ſame interval.

Exp. 25. Metal covered with gold-beater's ſkin cools faſter.

Exp. 26. Both inſtruments were ſuffered to remain in the cold all night, when the temperature in the naked inſtrument was 50⅛°, and that of the covered inſtrument 49¼°; the air of the room being 48°. Both inſtruments were then removed into a warm room, of which the temperature continued be-

Exp. 26. And alſo acquires heat more quickly.

tween 64° and 65°. The covered inſtrument acquired heat the moſt rapidly; for in a quarter of an hour both ſtood at 51½°, and at the end of four hours the naked inſtrument ſhewed the temperature 61°, and the covered 63½°: whence the Count obſerves, that thoſe ſubſtances which part with heat the moſt readily, have alſo the greateſt facility to acquire it.

Rays received as well as thoſe emitted, alter the heats of bodies.

He then proceeds to reaſon on the probability that the temperatures of bodies may be changed, not only by the rays they emit, but by thoſe they receive from other bodies; and as the cooling of hot bodies is ſo much accelerated by covering their ſurfaces with ſubſtances which affect the radiation or abſorption, he thinks it highly probable that the air is but little affected as to its temperature, by theſe rays which paſs through it; and he contemplates the eſtabliſhment of this ſuppoſition, as promiſing to explain various intereſting phenomena; particularly that of the ſteady temperature of living animals, notwithſtanding the great quantity of heat generated in their lungs, and the different temperatures of the fluid which ſurrounds them.

Radiation does not heat the air.

Steady temperature of animals,

may be affected by radiation.

For it is evident, the greater power an animal may poſſeſs of throwing off heat by radiation, independently of the effect of the contact of the ſurrounding air, the leſs will his temperature be affected by the changes in that fluid, or the oppreſſive heat of the climate. The probability that negroes and people of colour, who ſupport the heats of tropical climates much better than white people, offers itſelf in this place as the conſequence of their colour; a quality which not only enables them to throw off heat, but even, as the Count much ſuſpects, to abſorb frigorific rays from ſuch bodies as may emit them.

Inſtance in negroes.

Exp. 27. Black paint upon animal membrane increaſes radiation. Emiſſion,

Exp. 27. When the flat ends of both the horizontal cylindrical veſſels were covered with gold-beater's ſkin, and one of them painted black upon the covering with Indian ink, this laſt, as indicated by the thermoſcope, emitted more calorific rays from the included hot water than the other veſſel did.

and alſo reception.

Exp. 28. When the ſame two veſſels filled with boiling-hot water, were ſet to cool, the blackened veſſel cooled through the ſtandard interval of ten degrees in 23¾ minutes, while the other, which was not blackened, employed 28 minutes.

The author again reverts to the application of theſe facts to animal bodies. Whether the oily coating which ſavages apply to their ſkins in cold climates, may not add to their comfort by reflecting frigorific rays; whether the Hottentots, ſtill more diſguſtingly beſmeared, may not derive advantages ſimilar to thoſe derived by negroes from their black colour? are queſtions that promiſe to lead to reſults of practical value. He then proceeds to explain more fully the manner in which negroes may be ſuppoſed to reſiſt the action of a burning ſun. An oil exudes from the ſkin of theſe people when expoſed naked to the ſun; which oil reflects the ſun's calorific rays. Heat more intenſe produces ſweat, which not only aids the former proceſs, but generates cold by evaporation. But when the ſun is ſet, the oil retires from the ſurface, and the ſkin becomes well adapted to admit frigorific rays from the neighbouring bodies.

Effects of the oil on the ſkins of Hottentots, &c. and of the natural changes in the ſkins of negroes.

Exp. 29. The thermoſcope was perceptibly affected by the radiation of cold bodies: It was deſirable to know whether this effect would be ſhewn in a groſſer way, by accelerating the cooling of a hot body. For this purpoſe, two conical veſſels of thin ſheet braſs, each four inches diameter at the baſe, and four inches high, ending above in a cylindrical neck, were ſeparately encloſed in a cylinder of thin paſteboard covered with gilt paper, and then the veſſels were covered up with rabbit-ſkins having the hair on them, in ſuch a manner that no part of theſe veſſels, except their flat bottoms, was expoſed naked to the air. The bottoms were covered with gold-beater's ſkin painted black with Indian ink, in order to render them as ſenſible as poſſible to calorific and frigorific rays.

Exp. 29. Veſſ of water more rapidly cooled by radiation from ice.

The two veſſels thus prepared were ſuſpended, with their bottoms downwards from the arms of a ſtand, and under each was placed a pewter platter blackened on the inſide by ſmoke from a candle. The platters themſelves were ſupported on ſhallow earthen diſhes which reſted upon wooden ſtands; each pewter platter having a perforated cover of thick paper, in the center of which was a hole ſix inches in diameter. The diſtance from the floor of the room to the ſmoked ſurface of each platter was 40 inches, and the interval between the ſurface of each conical veſſel and its correſpondent platter immediately beneath, was four inches. One of the platters remained at the temperature of the room, but the other was kept conſtantly ice cold by means of pounded ice and water contained in the diſh beneath it.

The two conical veſſels were now filled with boiling hot water, and every precaution was taken to prevent agitation in the air; the only means by which an aſcent of cold fluid could be ſuſpected. The reſult of the experiment was, that the veſſel ſuſpended over the ice-cold platter cooled from 50° to that of 40° above the temperature of the room, in 33 minutes and 42 ſeconds; whereas the other veſſel required 39 minutes and 15 ſeconds to cool through the ſame interval.

Exp. 30. Repetition.

Exp. 30. Upon repeating this experiment the next day, the times were 33 minutes 15 ſeconds and 39 minutes 30 ſeconds.

Velocity of cooling accelerated by the radiation from ice, in the ratio of 5 to 4.

As the cooling of theſe veſſels is a complicated proceſs which includes the conſideration of the heat that paſſed through the covered ſides, in theſe different times; the Count enters into a proceſs of computation, founded on the principles made uſe of at page 61, by which he determines that the velocity with which the heat paſſed through the bottom of the veſſel expoſed to the ice, was to that with which it paſſed through the bottom of the other veſſel as five to four nearly.

Exp. 31. Proceſs to ſhew the heat abſorbed by the air when a current was allowed to take place. It was then one twenty-ſeventh of the whole loſs.

Exp. 31. By the manner of defending the two conical veſſels each had a circular band or hoop of the fine poſt paper which projected half an inch below its baſe. It is evident that the air in this ſpace could not paſs upwards, as it became heated by contact, and conſequently, that very little of the cooling effect could have been produced by the contact of an aſcending current. To aſcertain what this effect might have been, the two veſſels were ſuſpended as before; but one of them had its baſe inclined in an angle of 45°, while the other baſe continued horizontal. In this ſituation they were filled with boiling water, and ſuffered to cool without the platter and ſtands beneath them. Two effects would follow from theſe arrangements: the veſſel, which in the other experiment had been placed over a platter at the temperature of the room, would now cool a very little faſter by the abſence of that platter, which no doubt muſt have had its temperature a little raiſed; and the inclined veſſel in the preſent experiment would be cooled ſomewhat more ſpeedily, by the ſucceſſive contact of the aſcending current of air which was at liberty to riſe. This veſſel was found to cool through the ſtandard interval of 10° in 37¼ minutes, and the horizontal veſſel employed 38½ minutes. By referring theſe facts to computation, the Count finds that the heat loſt by actual communication to the air, is nearly $\frac{1}{17}$ part of the whole loſs.

Exp. 32. Velocity of cooling retarded by the vicinity of other bodies. Hence clothing.

Exp. 32. The counter radiation by the platter, which was ſtated under the laſt experiment as impeding the cooling proceſs, affords the important proſpect of explaining the effect of clothing, and therefore deſerved to be more fully examined. The experiments 29 and 30 were therefore repeated, with the diſtance of three inches only between the bottom of each veſſel and its correſponding platter. The times of cooling through 10°, were now 33¼ minutes and 40¼ minutes.

Exp. 33. Repetition.

Exp. 33. And when the diſtance was diminiſhed to two inches, the times proved 32½ minutes and 42¾ minutes.

The vicinity of many thin bodies in ſucceſſion will conſtitute a clothing;

Theſe experiments ſhew that the vicinity of a cold body, of which the low temperature is not kept up by artificial means, retards the cooling of a hot body. And from this fact the Count concludes, that if the hot body had been a globe ſuſpended in the center of another larger thin hollow ſphere, of the ſame temperature, at the commencement, as the air and walls of the room, the cooling would have been more ſlow than if the external globe had not been preſent; and alſo, if the external globe itſelf were included in another globe of the ſame deſcription, the retardation would have been ſtill more conſiderable. And by extending this ſuppoſed experiment to a number of thin concentric hollow ſpheres, we may conceive a great retardation to follow, and ſhall become acquainted with the nature of the effects which take place when a hot body is ſurrounded by proper clothing.

more effectual if the ſurfaces be poliſhed.

If the ſpheres were metallic, the cooling would be ſlower if the ſurfaces were poliſhed than if unpoliſhed or blackened; whence it is highly probable, that the warmth of any clothing depends very much upon the poliſh of its ſurface.

Furs, feathers, ſilks, &c. are highly poliſhed.

The microſcope ſhews that thoſe ſubſtances which ſupply us with the warmeſt coverings, ſuch as furs, feathers, ſilks, and the like, have their ſurfaces highly poliſhed; and the finer the fibres, or the greater number of interpoſed poliſhed ſurfaces, the warmer is the clothing.

Clothing defends much more by radiation than by preventing circulation of the air.

In the former experiments of Count Rumford, he conſidered the warmth of clothing as principally depending on the obſtacles it oppoſes to the motion of the ſurrounding cold air;

but, by a patient and careful examination of the ſubject, he is convinced that the efficacy of radiation is much greater than he had ſuppoſed: from the reſult of the experiment No. 31, it appears that a very ſmall part of the heat of a body cooled in the air, is in fact communicated to that fluid; much the greateſt portion being communicated to ſurrounding bodies at a diſtance; and, in one of his former experiments, a hot body was cooled, though it was placed in a torricellian vacuum.

Air heated only at its ſurface and by radiation.

He conſiders the heat which air receives by coming in contact with a hot body, to be communicated by radiation, in the ſame manner as it is received by other bodies at a greater diſtance; and he apprehends that the contiguous particle receives the heat in preference for no other reaſon than becauſe it is at the ſurface of the fluid, this being the place where reflection, refraction, and increaſe of temperature, take place; and, from theſe conſiderations, he explains what has been called the non-conducting power of tranſparent fluids.

Analogy between light and heat.

By extending the analogy of thoſe facts which we know concerning the effect of poliſhed ſurfaces on light, to the radiations of heat alſo, the preceding facts are eaſily explained. The frigorific rays are reflected externally, and a large portion of the calorific rays, which would have iſſued forth through a rough ſurface, are, in the other caſe, turned inwards by reflection.

A drop of water reſiſts the heat of ignited iron, by its poliſh.

The poliſhed ſurface of a drop of water, which rolls about at a diſtance from the face of a piece of red-hot iron, enables it to reflect the calorific rays; the water acquires little heat, and evaporates ſlowly.

With a leſs heat the water enters the pores of the oxide upon the metal, loſes its poliſh, acquires heat very rapidly, and is ſoon evaporated.

It retains its poliſh longer in ſilver, and reſiſts more.

If the metal be leſs oxidable, as, for example, a ſilver-ſpoon, the drop of water will ſupport or reſiſt a lower heat. In fact it does not ſo ſoon loſe its poliſh; but at a ſtill lower heat, that is to ſay, a little above boiling-water, a drop of water is inſtantaneouſly evaporated.

Exp. 34. Water rolling in a ſpoon blackened with ſmoke, cannot be made to boil.

Exp. 34. A clean poliſhed ſpoon, rendered black by holding it over the flame of a wax candle, will receive a large drop of water, which will roll about without wetting the blackened ſurface. This drop cannot be made to boil by holding the ſpoon over the flame of a candle. When the ſpoon is too hot to be held by its handle, the drop of water poured into the palm of the hand is warm, but by no means ſcalding hot.

Exp. 35. Drop of water put in to the flame of candle, is not heated.

Exp. 35. If a large drop of water be formed at the end of a ſmall ſplinter of light wood, and the drop be thruſt quickly into the center of the flame of a newly ſnuffed candle, it will remain for a conſiderable time in the center of the flame, without being apparently affected by the heat; and if it be taken out of the flame and put upon the hand, it will not be found to be ſcalding hot. If it be held for ſome time in the flame, it will be gradually diminiſhed by evaporation; but it does not appear that the heat is communicated by the flame, but by the wood to which it adheres, which is ſoon heated, and at laſt ſet on fire.

Why the theoretical remarks are not here abridged.

The remainder of the Count's memoir conſiſts of theoretical remarks and inferences, occupying 29 pages of the Tranſactions. I have endeavoured faithfully to deſcribe the facts in the way of abridgement, but cannot with the ſame facility do juſtice to theſe argumentative reſults. I ſhall therefore, for the preſent, conclude my account of his paper; but may not perhaps wholly overlook his theories upon ſome future occaſion.

XIII.

Letter from Profeſſor VEAU-DE-LAUNAY *to* J. C. DELAMETHERIE, *on fulminating Silver**.

Accident with fulminating ſilver.

AS it is at all times uſeful to ſtate facts, whatever may be the reſults, I think it right to inform you of an accident which occurred in my laboratory.

I had employed one of my pupils, a very good operator, to prepare a ſmall quantity of fulminating ſilver, which he executed with ſkill.

The quantity obtained was about five grains, or a quarter of a gramme: it was depoſited in a cryſtal capſule about two lines in thickneſs. He had taken a ſmall quantity, about half a grain, which was ſeparated with a card, and then dried, and afterwards detonated by ſlight friction. Next day, that is to ſay twenty-four hours after the preparation, this young perſon was deſirous of taking an equal quantity from the capſule to repeat the experiment, but he had ſcarcely touched the preparation with the corner of a card when a violent detonation and exploſion took place in the capſule which was ſhattered into a thouſand pieces. His face was covered with the vaporiſed preparation, which was almoſt black, and adhered ſtrongly to the ſkin: his eyes experienced a ſtrong ſhock, which produced extreme pain; the opaque cornea became red and inflamed. Happily his fear was the greateſt evil: by waſhing and bathing his eyes and face frequently with cold water, the effects of the detonation were ſoon diſſipated.

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* From Journal de Phyſique, &c. Floreal, An. XII.

Fortunately, none of the fragments of the glaſs had touched his eyes or his face; they were thrown nearly in a horizontal direction, to a conſiderable diſtance: Some were thrown upwards of twelve feet.

As the effects of this preparation may have more calamitous conſequences. I think it uſeful to be guarded againſt the dangers which it may occaſion.

XIV.

*Pyrotechnic Obſervations, with their Application to evaporating Furnaces**. *By Cit.* CURAUDAU, *Correſponding Member of the Apothecaries Society of Paris, and Reſident Aſſociate at the Atheneum of Arts.*

SINCE I publiſhed my obſervations on the cauſes of the imperfections of evaporating furnaces, I have had occaſion to make others on the ſame ſubject, the application of which may add to the advantages in their conſtruction which I have made known.

Diſadvantages in the conſtruction of evaporating furnaces, &c.

In my firſt memoir, I proved that the bottom of the copper in the evaporating furnaces, not only obſtructs the elevation of the temperature, but alſo diminiſhes the activity of the fire, and is rather favourable to the gazification of the combuſtible body, than to its oxigenation: I cited the lamp of Argand with its glaſs chimney, as an example of the neceſſity of raiſing the temperature round the combuſtible, whenever a compleat and powerful combuſtion is required. At preſent, I ſhall take the enameller's lamp as an example in ſupport of my new obſervations: I may ſay that I am indebted to the examination of its effects, for thoſe which it has led me to make. In fact, if the jet of the flame of an enameller's lamp be examined, it will be found that the intenſity of its action depends on the current of air which is directed on the flame of the wick; it will alſo be ſeen, that it is only at the extremity of its jet that the greateſt energy of the calorific rays exiſts, and that its intenſity is ſuch that, by means of this lamp, effects may be produced, which are, comparatively, as powerful as thoſe obtained in our beſt furnaces.

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* From Annales de Chimie, No. 149, Floreal, An. XII.

Effects of the current of air on the flame of Argand's lamp,

This mode of action of the caloric proves, therefore, that its effects may be augmented, by augmenting the rapadity of its current, and by directing it ſkilfully upon the body to be heated. Theſe are the conditions which I have endeavoured to unite in my new conſtruction, and which, agreeably to the application I have made of them, are employed to ſupport an opinion which required an experiment on a large ſcale to eſcape being placed in the claſs of hypotheſes.

Having been lately conſulted upon the conſtruction of a brewer's furnace, I took the opportunity to ſhow the great advantages of the alterations which my obſervations appeared to me to render neceſſary, and to induce the proprietor to conſtruct his furnace according to the plan I ſent him.

applied to works on a large ſcale.

The following is the reſult of the experiments which were made to aſcertain tbe advantages poſſeſſed by the new furnace over that which it replaced.

Compariſon of two furnaces.

The old furnace required 2¾ hours to raiſe the temperature of 2500 litres of well-water to 50° of Reaumur, and conſumed in the operation, which was repeated daily, 625 kilogrammes of new dry wood.

The preſent furnace, on the contrary, conſumes only 450 kilogrammes of wood in the ſame operation, and is only one hour in raiſing the temperature of 2500 litres of well-water to 50°: Whence it reſults, that this new conſtruction evidently makes a ſaving of 7/11 in time and nearly one third in the combuſtible.

Such advantages appeared to me to be of ſufficient importance to deſerve to be known, and to make it deſirable that advantage may be taken of a new proceſs which will have great influence on the economy of the fuel neceſſary in manufactures.

Deſcription of the Furnace. Plate

Deſcription of the new furnace. A. Opening of the fire-place: it is 16 inches wide and 13 high.

B. This part of the furnace reſembles a vault: It is 21½ inches in height, 31½ in width, and 5 feet in depth. To add to the effect produced by the heat, I give the middle of the fire place a depth of 2 inches more that at its ſides, which makes it preceptibly concave.

C. An opening made in the middle of the vault, and which is intended to increaſe the rapidity and the action of the calorific rays; it is 5 inches high, and is equal in thickneſs to the vault. In its lower part, this opening is 23½ inches long, and 17½ wide; and, in its upper part, is 19½ long, and 13½ wide, which gives each extremity of the opening the form of a ſpheroid whoſe longeſt axis is in the direction of the length of the vault.

D. The diſtance of the copper from the orifice for the heat is 5 inches in the middle, and is reduced to 4, at the angle E, which gives the advantage of concentrating the calorific rays, in proportion as they loſe their intenſity by their expanſion.

From the angle E to that at F, there is a diſtance of 15 inches: the re-entering angle of F is 2¼ inches from the copper, and its ſaliant angle only one inch.

G and H are angles ſimilar to that at F, but which are 11 inches diſtant from each other: theſe angles may be multiplied according to the height of the copper. The advantages which they give conſiſt in making the calorific ſtream undergo ſeveral breaks, which increaſes their power at the place of their deviation.

I is an opening communicating with the chimney: it is 27 inches wide and 5 inches high. At the angle H, half the circumference of the furnace ſhould be cloſed by a row of bricks, for the purpoſe of forcing the heat to direct itſelf from the ſide oppoſite to the chimney; immediately above this courſe of bricks, thoſe which ſucceed muſt be removed four inches from the copper, and continue ſo to the height of four inches; afterwards each courſe of bricks muſt be brought nearer, ſo that this reſervoir of heat may be cloſed at the height of five inches, which muſt be done round all the circumference of the copper.

K is the opening of the chimney: it muſt be three decimetres ſquare through all its height, and at leaſt four metres high.

L is a blower of hammered iron; it is one metre above the copper, and ſerves to open or cloſe the chimney at pleaſure.

The proportions laid down in this plan are intended for a copper four feet three inches wide in its lower part, and forty inches deep.

XV.

An Account of a curious Phenomenon obſerved on the Glaciers of Chamouny; together with ſome occaſional Obſervations concerning the Propagation of Heat in Fluids. By BENJAMIN *Count of* RUMFORD, *V. P. R. S. Foreign Aſſociate of the National Inſtitute of France, &c. &c.* *

Cylindrical pit (in a maſs or ſea of ice) containing water.

IN an excurſion which I made the laſt ſummer, in the month of Auguſt, to the Glaciers of Chamouny, in company with Profeſſor Pictet of Geneva, I had an opportunity of obſerving, on what is called the Sea of Ice, *(Mer de Glace,)* a phenomenon very common, as I was told, in thoſe high and cold regions, but which was perfectly new to me, and engaged all my attention. At the ſurface of a ſolid maſs of ice, of vaſt thickneſs and extent, we diſcovered a pit, perfectly cylindrical, about ſeven inches in diameter, and more than four feet deep; quite full of water. On examining it on the inſide, with a pole, I found that its ſides were poliſhed; and that its bottom was hemiſpherical, and well defined.

This pit was not quite perpendicular to the plane of the horizon, but inclined a little towards the ſouth, as it deſcended;

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* Phil. Tranſ. 1804, p. 23.

ſcended; and, in conſequence of this inclination, its mouth or opening, at the ſurface of the ice, was not circular, but elliptical.

They are frequently found.

From our guides I learnt, that theſe cylindrical holes are frequently found on the level parts of the ice; that they are formed during the ſummer, increaſing gradually in depth, as long as the hot weather continues; but that they are frozen up, and diſappear, on the return of winter.

Inference againſt the conducting power of water.

I would aſk thoſe who maintain that water is a conductor of heat, how theſe pits are formed? On a ſuppoſition that there is no direct communication of heat between neighbouring particles of that fluid, which happen to be at different degrees of temperature, the phenomenon may eaſily be explained; but it appears to me to be inexplicable on any other ſuppoſition.

The quieſcent maſs of water, by which the pit remains conſtantly filled, muſt neceſſarily be at the temperature of freezing; for it is ſurrounded on every ſide by ice: but the pit goes on to increaſe in depth, during the whole ſummer. From whence comes the heat that melts the ice continually at the bottom of the pit? and how does it happen, that this heat acts on the *bottom* of the pit only, and not on its ſides?

Solution of the effect by the greater denſity of water above zero.

Theſe curious phenomena may, I think, be explained in the following manner: The warm winds which, in ſummer, blow over the ſurface of this column of ice-cold water, muſt undoubtedly communicate ſome ſmall degree of heat to thoſe particles of the fluid with which this warm air comes into immediate contact; and the particles of the water at the ſurface ſo heated, being rendered ſpecifically heavier than they were before, by this ſmall increaſe of temperature, ſink ſlowly to the bottom of the pit; where they come into contact with the ice, and communicate to it the heat by which the depth of the pit is continually increaſed.

This operation is exactly ſimilar to that which took place in one of my experiments, (See my Eſſay on the Propagation of heat in Fluids, *Experiment* 17,) the reſults of which, no perſon, to my knowledge, has yet explained.

Conſtant temperature of water at the bottom of deep lakes, offered in ſupport of the non-

There is another very curious natural phenomenon, which I could wiſh to ſee explained in a ſatisfactory manner, by thoſe who ſtill refuſe their aſſent to the opinions I have been led to adopt, reſpecting the manner in which heat is propagated in fluids. The water at the bottoms of all deep lakes is conſtantly at the ſame temperature, (that of 41° Fahrenheit,) ſummer and winter, without any ſenſible variation. This fact alone appears to me to be quite ſufficient to prove, that if there be any immediate communication of heat between neighbouring particles or molecules of water, *de proche en proche*, or from one of them to the other, that communication muſt be ſo extremely ſlow, that we may with ſafety conſider it as having no exiſtence; and it is with this limitation that I beg to be underſtood, when I ſpeak of fluids as being non-conductors of heat.

conducting property of fluids.

In treating of the propagation of heat in fluids, I have hitherto confined myſelf to the inveſtigation of the ſimple matter of fact, without venturing to offer any conjectures relative to the cauſes of the phenomena obſerved. But the reſults of recent experiments on the calorific and frigorific radiations of hot and of cold bodies, (an account of which I ſhall have the honour of laying before the Royal Society in a ſhort time,) have given me ſome new light reſpecting the nature of heat, and the mode of its communication; and I have hopes of being able to ſhow *why* all changes of temperature, in *tranſparent* liquids, muſt neceſſarily take place at their ſurfaces.

I have ſeen with real pleaſure, that ſeveral ingenious gentlemen, in London, and in Edinburgh, have undertaken the inveſtigation of the phenomena of the propagation of heat in fluids; and that they have made a number of new and ingenious experiments, with a view to the farther elucidation of that moſt intereſting ſubject. If I have hitherto abſtained from taking public notice of their obſervations on the opinion I have advanced on that ſubject, in my different publications, it was not from any want of reſpect for thoſe gentlemen that I remained ſilent, but becauſe I ſtill found it to be quite impoſſible to explain the reſults of my own experiments, on any other principles than thoſe which, on the moſt mature and diſpaſſionate deliberation, I had been induced to adopt; and becauſe my own experiments appeared to me to be quite as concluſive (to ſay no more of them) as thoſe which were oppoſed to them; and, laſtly, becauſe I conſidered the principal point in diſpute, relative to the paſſage of heat in fluids, as being ſo clearly eſtabliſhed by the circumſtances attending ſeveral great operations of nature, that this evidence did not appear to me to be in danger

Notice of experiments againſt this doctrine.

danger of being invalidated by concluſions drawn from partial and imperfect experiments, and particularly from ſuch as are allowed on all hands to be extremely delicate.

Heat of the ſides of the veſſel, and deſcending current in a veſſel of ice.

In all our attempts to cauſe heat to deſcend in liquids, the heat unavoidably communicated to the ſides of the containing veſſel, muſt occaſion great uncertainty with reſpect to the reſults of the experiment; and, when that veſſel is conſtructed of ice, the flowing down of the water reſulting from the thawing of that ice, will cauſe motions in the liquid, and conſequently inaccuracies of ſtill greater moment, as I have found from my own experience; and, when thermometers immerſed in a liquid, at a ſmall diſtance below its ſurface, acquire heat, in conſequence of a hot body being applied to the ſurface of the liquid, that event is no deciſive proof that the heat acquired by the thermometer is communicated by the fluid, from above, downwards, from molecule to molecule, *de proche en proche;* ſo far from being ſo, it is not even a proof that it is from the fluid that the thermometer receives the heat which it acquires; for it is poſſible, for aught we know to the contrary, that it may be occaſioned by the radiation of the hot body placed at the ſurface of the fluid.

Reference to the experiments of boiling water ſtanding over ice.

In the experiments of which I have given an account, in my Eſſay on the Propagation of Heat in Fluids, great maſſes, many pounds in weight, of boiling hot water, were made to repoſe for a long time (three hours) on a cake of ice, without melting but a very ſmall portion of it; and, on repeating the experiment with an equal quantity of very cold water, (namely, at the temperature of 41° Fahrenheit,) nearly twice as much ice was melted in the ſame time. In theſe experiments, the cauſes of uncertainty above mentioned did not exiſt: and the reſults of them were certainly moſt ſtriking.

The concluſions which naturally flow from thoſe reſults, have always appeared to me to be ſo perfectly evident and indiſputable, as to ſtand in no need, either of elucidation, or of farther proof.

If water be a conductor of heat, how did it happen that the heat in the boiling water did not, in three hours, find its way downwards, to the cake of ice, on which it repoſed, and from which it was ſeparated only by a ſtratum of cold water, half an inch in thickneſs?

Recapitulatory arguments.

I wiſh that gentlemen who refuſe their aſſent to the opinions I have advanced reſpecting the cauſes of this curious phenomenon, would give a better explanation of it than that which I have ventured to offer. I could likewiſe wiſh that they would inform us how it happens, that the water at the bottoms of all deep lakes remains conſtantly at the ſame temperature: and, above all, how the cylindrical pits, above deſcribed, are formed in the immenſe maſſes of ſolid and compact ice which compoſe the Glaciers of Chamouny!

Notice of one remark of Dr. Thompſon.

A remark, which ſurpriſed me not a little, has been made by a gentleman of Edinburgh, (Dr. Thompſon,) on the experiments I contrived, to render viſible the currents into which liquids are thrown on a ſudden application of heat, or of cold. He conceives, that the motions obſerved in my experiments, among the ſmall pieces of amber which were ſuſpended in a weak ſolution of potaſh in water, were no proof of currents exiſting in that liquid; as they might, in his opinion, have been occaſioned by a change of ſpecific gravity in the amber, or by air attached to it. I am ſorry that ſo mean an opinion of my accuracy as an obſerver ſhould have been entertained, as to imagine that I could have been ſo eaſily deceived. For nothing ſurely is eaſier, than to diſtinguiſh the motion of a ſolid ſuſpended in a liquid of the ſame ſpecific gravity, which is carried along by a current in the liquid, from that of a body which deſcends, or aſcends, in the liquid, in conſequence of its relative weight, or levity. In the one caſe, the motion is uniform; in the other, it is accelerated. In a current, the body may be carried forward in all directions, and even in curved lines; but, when it falls in a quieſcent fluid, by the action of gravity, or riſes, in conſequence of its being ſpecifically lighter than the fluid, it muſt neceſſarily move in a vertical direction.

The fact is, that I very often obſerved, in the courſe of my numerous experiments, the motions of ſmall particles of matter, of different kinds, in water, which Dr. Thompſon deſcribes; but, ſo far from inferring *from them* the exiſtence of currents in that fluid, their cauſe was ſo perfectly evident, that I did not even think it neceſſary to make any mention of them.

I cannot conclude this Paper, without requeſting that the Royal Society would excuſe the liberty I have taken in troubling them with theſe remarks. Very deſirous of avoiding every ſpecies of altercation, I have hitherto cautiouſly abſtained from engaging in literary diſputes; and I ſhall moſt certainly endeavour to avoid them in future.

I am reſponſible to the public for the accuracy of the accounts which I have publiſhed of my experiments; but it cannot reaſonably be expected, that I ſhould anſwer all the objections that may be made to the concluſions which I have drawn from them. It will however, at all times, afford me real ſatisfaction to ſee my opinions examined, and my miſtakes corrected; for my firſt and moſt earneſt wiſh is, to contribute to the advancement of uſeful knowledge.

XVI.

Account of two Sketches; viz. *one for a perpetual Motion, and the other of a jointed Parallel Rule, which has no ſide Deviation. In a Letter from* R. B.

To Mr. NICHOLSON.

SIR,

Perpetual motions deſcribed in the Philoſophical Journal.

I was much gratified ſeveral years ago by ſome eſſays with which you obliged your readers upon the perpetual motion. In the firſt volume, p. 375, of your quarto ſeries, I find an account of ſeveral ſchemes (neceſſarily abortive) for producing perpetual motions by the action of gravity, and in your third volume, I find an account of various methods of keeping up the motion of a machine by means of the changes which take place in the barometer and thermometer. I have ventured to ſend you the ſketch of a project for a perpetual motion of the latter kind, which has long remained among my memorandums. You will ſee that it is not of the claſs of perpetual motions properly ſpeaking, but merely the application of ſome exiſting intermittent motions in nature to the purpoſe of maintaining the rotation of machinery. If you ſhould heſitate about inſerting this, I think the other ſketch which accompanies it of an uſeful inſtrument, cannot fail to meet your approbation.

Deſcription of a wheel kept in motion by the marine barometer.

Fig. 1, *Plate XII.* A repreſents the marine barometer of Halley, but varied by the addition of a veſſel B at the open end, in which the water or other fluid, expoſes a ſurface nearly equal to that in the cloſed veſſel A. Theſe two veſſels are connected by a long horizontal tube G. It is evident that any change, either in the preſſure of the external air or the elaſticity of the internal air, will cauſe the fluid to run along the tube, and add to the weight of A or of B according to circumſtances. The heavier veſſel will preponderate; but it will be prevented from deſcending too far by a ſtop or bearing to which it will arrive. Any change in the inclination of G will move the attached lever C D; by means of which, one of the two horizontal racks will be made to puſh round that ratchett wheel into which its teeth fall, at the ſame time that the other rack will be drawn backwards upon its wheel. The oppoſite action will drive forward the other wheel; and as both theſe wheels are fixed on the ſame axis, the ſyſtem will be driven the ſame way by every change of denſity or weight in the air that takes place.

Account of a new parallel rule.

The other inſtrument, *Fig.* 3, is a parallel rule which is framed without any ſliding work, and opens to a much greater extent than uſual, without any ſide deviation. I do not know the inventor; but it was communicated to me by a private hand. The jointed parts are repreſented in *Fig.* 2, and the dimenſions are expreſſed by the ſmall numerals. From the joint D to the joint E upon the bar, C E, the diſtance, is = 1; from the joint E to the joint A, = 2; from the joint A to the joint B, = 3; and from the joint B to the joint D, = 4; which is alſo equal to the part D C of the Bar E C. Now it is found, and I muſt leave the mathematical proof to your readers, that the point C moves by the opening from B (upon which it lies when ſhut) in a ſtrait line, very nearly at right angles with B A. From this property it is eaſily ſeen, that two parallel rules, having the ſyſtem of four bars fixed to each, and connected as in the figure, will be moveable without ſide deviation, and will preſerve their paralleliſm in all ſituations.

I am Sir,

Your obliged humble ſervant,

R. B.

Familiar

XVII.

Familiar Account of the Method of eſtimating the Value of a Steam-Engine in Horſe-Powers as they are called. By a Correſpondent.

To Mr. NICHOLSON.

SIR,

Introductory letter.

AS your excellent Journal is the repoſitory for uſeful information, whether ſcientific or practical, I have thought I ſhould oblige many manufacturers and others of your readers, by ſending you a very clear report about horſe-powers, which a friend of mine has communicated to me, and was received by him from an eminent character in anſwer to an enquiry profeſſionally made.

It clearly appears from this paper, that the calculation by horſe-powers muſt be fallacious, unleſs engineers could agree as to the quantity of work they would arbitrarily, in the firſt inſtance, aſcribe to one horſe; and then the expreſſion would be nugatory. And not only ſo, but it would not then be true that the value of a ſteam-engine in work, however clearly expreſſed in quantity *per* day, would be fairly ſhewn, unleſs the wages or food of the working being were taken into the account. Coals may be ſtiled the food of a ſteam-engine, and nothing is more evident than that, if two engines raiſe equal quantities of water *per* hour, but conſume different quantities of coal, they will not be equally beneficial to the proprietors. I would therefore propoſe, that the eſtimate ſhould be made by attending to theſe two particulars only, and ſaying nothing about horſes, at leaſt in ſpecific arguments intended to have legal effects: And, as a ſupplement to the facts and obſervations contained in the report, I will add, that one of the beſt engines of Boulton and Watt, has been known to raiſe between 28 and 30 millions of pounds of water to the height of one foot with one buſhel of good coals, which appears to be an outſide meaſure; and that, though there are ſubſequent improvements both in the conſtruction of furnaces and the working gear, yet there are ſome among late engines which fall ſhort of 20 millions.

I am, Sir,
Your conſtant reader,
E. T.

Report

*Report concerning the Power of a Steam-Engine erected by Contract at * * * *.*

Steam engin
* * * *.

IT is required to determine whether the ſteam-engine erected at * * * * by Mr. ——, be equal to the power of ſixteen horſes. The ſame has a cylinder of 21½ inches diameter, and gives 23 double ſtrokes *per* minute, of four feet each.

Reaſon why
ſteam engine
have been co
pared with
horſes.

It is an uncer
method.

In anſwer to this queſtion it muſt be previouſly remarked, that ſteam-engines having originally been recommended and ſubſtituted inſtead of horſes, the method of computing by the number of thoſe animals intended to be ſupplied by means of this invention, has been generally applied, though it is much leſs certain and accurate than other methods well known to mechanical men. The uncertainty of calculating by horſe-powers ariſes from various cauſes; ſuch as the great differences of ability between the ſtrong and heavy horſes uſed in London, and thoſe of not half the ſtrength uſed in various parts of the country; the greater or leſs degree of ſpeed during work; the quantity of re-action againſt which they are urged to pull; the ſhorter or longer time of work; their food, ſtabling, &c. &c. And this uncertainty, as may eaſily be conceived, is ſo great, that the words *horſe-power* cannot practically be applied, otherwiſe than to denote a certain quantity of mechanic effect agreed upon and underſtood between engineers, and muſt not be underſtood to denote any elementary meaſure, capable of being worked out or inferred within any reaſonable or uſeful limits, from the real power of the horſe himſelf.

Words horſe-
power muſt d
note an arbitr
quantity of
effect.

It therefore follows of neceſſity, that the engine muſt be examined by firſt ſtating its mechanic effect; that is to ſay, how many pounds weight it is capable of raiſing through a given ſpace in a given time, that is to ſay, through the height of one foot during one minute, and then dividing this ſum by the like effect producible by one horſe, according to the ſtatements and practice of engineers of the firſt reputation. I confine myſelf to Meſſ. Boulton and Watt, Dr. Deſaguliers and Mr. Smeaton.

Quantities of
work per horſ
by Boulton an
Watt, Deſagu-
liers and Smea
ton.

The practice of Meſſ. Boulton and Watt is, to conſider a horſe as capable of raiſing a certain weight, which is ſtated to be of 32000 pounds avoirdupois, one foot high in one minute. Deſaguliers' reſults brought to the ſame form, give 27500 pounds; and Smeaton's 22916 pounds, under the ſame circumſtances. The loweſt of theſe performances are more than equal to the average power of a horſe employed in huſbandry for eight hours *per* day.

A ſteam engine computed.

If the diameter of the cylinder be multiplied by itſelf (21⅛ × 21⅛), the product will give 446¼ round inches for the whole ſurface: and Smeaton reckons the effective or working preſſure *per* round inch on the atmoſpheric engine, at ſeven pounds avoirdupois. It is uſual to reckon the working preſſure on a cloſe engine (like that in queſtion), at 10 pounds the round inch; but I ſhall firſt take the ſeven pounds as being againſt the builder: So that, by multiplying the round inches 446¼ by 7, we have 3123¾ pounds for the weight raiſed. But the ſtrokes are 23 of four feet double *per* minute, that is to ſay, 184 feet. Multiply the weight 3123¾ by the height 184, and the product 574760 will be the mechanic effect of the engine, or the number of pounds it will raiſe one foot high in one minute. Laſtly, Divide this by Boulton and Watt's horſe-power ($\frac{574760}{32000}$), and the quotient 17$\frac{15}{16}$, or very nearly 18, will expreſs the power of the engine in horſes.

If we follow Deſaguliers, the engine will prove equal to 21 horſes.

And, according to Smeaton, its power will be equal to 25 horſes.

If we had taken the preſſure at 10 pounds *per* round inch, the powers would have proved much greater, as below.

Steam engine can work treble days.

In the above calculations the horſes are ſuppoſed to be fairly worked, and the engine is ſuppoſed to be ſtopped as ſoon as the horſes leave off. But an engine can work the whole 24 hours; and Smeaton, conſidering that three ſetts of horſes muſt be kept to work conſtantly for the ſame time, reckoned a ſteam engine to be equivalent to three times as many horſes as could equal its rate of working. The following table will ſhew the powers of this engine, according to all theſe ſeveral methods:

7 lb.

	Power in Horſes according to Boulton and Watt.	Ditto according Deſaguliers.	Ditto according to Smeaton.	24 hours work according to Smeaton in horſes.
7lb. preſſure per round inch.	$17\frac{26}{100}$ or 18 very nearly	21	25	75
10lb. preſſure per round inch.	25	30	$35\frac{3}{4}$	107

XVIII.

Experiments proving the Neceſſity of atmoſpherical Oxigen in the Proceſs of Vegetation. In a Letter from Mr. JOHN GOUGH.

To Mr. NICHOLSON.

SIR,

DR. Prieſtley and M. Ingenhouſz have made many experiments with plants, confined in azote; from which philoſophers have inferred, that vegetables differ from animals in not requiring the preſence and ſupport of atmoſpherical oxigen. One circumſtance however occurs in all theſe experiments, if my memory be correct, which renders the concluſion doubtful: the plants, uſed in them were in contact with the water that confined the azote in the inverted jars, through the medium of which they had evidently the ſame indirect communication with the atmoſphere that fiſhes are known to require.

Reflections on former experiments of the kind.

With a view to throw ſome additional light upon this diſputable point in the vegetable economy, I determined to ſurround certain ſucculent plants entirely with azote; which poſſeſs the ſingular property of vegetating when ſuſpended by the roots in dry air. My method of performing this conſiſts in fixing the ſubject of the experiment by means of a thread, to a ſmall perforated board, ſtanding on a pillar, which was furniſhed with a foot of lead. The plant being thus ſecured, was introduced through water into a glaſs jar of azote; where it was wholly ſurrounded by the gas.

Experiments on ſucculent vegetables.

Exp. 1. The firſt experiment, I made in this manner, was tried in June 1800. A plant of ſedum acre was ſurrounded by azote when it was ready to expand its flowers, and another was placed beſide it, under a ſimilar jar; which contained

Experiment wit ſedum acre goin to flower.

S common

common air. The latter ſpecimen flowered very well; but the former died away without opening one of its bloſſoms.

With the ſame in flower.

Exp. 2. The event was the ſame when I made uſe of a branch of this ſedum already in flower; for the plant withered and did not ſhed its petals; which is contrary to the uſual habits of this vegetable, when it is left to the impulſes of nature unthwarted by art.

With barren plants of the ſame and ſempervivum tectorum.

Exp. 3. Several plants of ſedum acre were alſo confined in jars filled with azote laſt ſpring, before the flower buds were formed upon them: not one of theſe attempted to vegetate; on the contrary they all withered in the courſe of four or five weeks. This was not the caſe with other ſpecimens, which were placed beſide them upon ſimilar ſtands, under jars containing common air and ſtanding in water; for they remained green and vigorous, after the former were dead; they had moreover produced a number of purple filaments ſpringing from the ſtalk; which is conſtantly done by the ſedum acre, when ſuſpended by the roots. It is proper to remark, that the air of the latter ſet was frequently changed, with a view to keep it near the ſtandard of the atmoſphere. The *ſempervivum tectorum* did not offer to vegetate when treated in the ſame manner by being confined in azote; but it appeared to be more tenacious of life under this kind of treatment, than the ſedum acre.

Inference from theſe experiments.

The preceding experiments induced me to conclude, at the time of making them, that the welfare of ſucculent plants requires them to be expoſed to the atmoſphere, and that they cannot grow when ſurrounded by azote, nor even preſerve the vegetative principle in it, for an indefinite length of time.

Seeds and bulbs in the ſame predicament.

I have ſhewn in the fourth volume of the Mancheſter Memoirs, that ſeeds and bulbs are not leſs indebted for their proſperity to the agency of oxigen, than ſucculent plants appear to be from the preſent letter. Thus it is manifeſt that the farther we carry our reſearches into the nature of organized bodies, the greater is the number of them; which are found to be incapable of performing their natural functions, when deprived of reſpirable air; but as the truth of this maxim remains to be extended experimentally to vegetables of a leſs humid conſtitution, than the ſedums, the following fact may be related with propriety.

Experiment made with ceraſtium vulgatum,

Exp. 4. A diminutive plant of *ceraſtium vulgatum* was placed on the 18th of February, 1804, upon a ſtand covered with a ſlice of wet ſponge; it was then put under an inverted jar, containing common air, and ſtanding in water. This plant encreaſed conſiderably in bulk in the courſe of 20 days; beſides which it had taken root in the ſponge; it was therefore transferred into a jar filled with azote; but this change of circumſtances put a ſtop to vegetation; for the herb made no further advances, but withered away.

with plants in water deprived of air.

Exp. 5. In order to diverſify theſe experiments as much as poſſible, I took a number of phials, having narrow necks, and filled them with water, which was either procured from ſnow, or deprived of air by boiling. Proper plants being then placed in the bottles, they were introduced quickly through water into jars filled with azote; under which circumſtances, all my ſpecimens died away ſooner or later without making one effort to vegetate. The herbs uſed on theſe occaſions were the common garden ſpear-mint and the draco-cephalum Moldavicum; both of which vegetate freely in bottles of water expoſed to the atmoſphere. I alſo treated in the ſame manner, a branch of Lyſimachia vulgaris, which was ready to flower; it declined gradually for a month, but never opened one bloſſom; though it is an aquatic plant and flowers very well in jars of water. It may here be remarked once for all, that vegetables die very ſlowly in azote, provided they are ſupplied with water deprived of air; but if my experiments may be relied on, all their natural functions ceaſe in theſe circumſtances, as ſoon as they are removed from the atmoſphere.

Plants die in azote for want of their proper ſtimulus.

Exp. 6. The loſs of energy experienced by plants confined in mephitic air, does not appear to ariſe from an injurious quality of the gas, but from the want of the ſalutary ſtimulus of oxigen; for a ſlip of ſpear-mint, which had remained twelve days in a glaſs of azote, recovered upon being reſtored to the air, with the loſs of its loweſt leaves only; and an off-ſet of ſempervivum vegetated freely after being removed from a jar of the ſame gas, in which I had kept it, from the 2nd of April to the 2nd of May, ſtanding in a bottle of ſnow water. Theſe facts ſeem to argue, that vegetables, treated in the manner deſcribed as above, ſuffer a ſpecies of torpidity, from which they may be recalled by the preſence of atmoſpherical air, provided the re-admiſſion of this neceſſary agent be not delayed too long; in which caſe, the plant periſhes, in conſequence of the continued inactivity of its organs, and the fermentation of its juices; which is occaſioned by the ſuſpenſion of all its functions.

Succulent plants abſorb air from the atmoſphere, but dry ones from water.

This fluid ſo neceſſary to the proceſs of vegetation, is evidently abſorbed from the atmoſphere by ſucculent plants, bulbs, and a variety of ſeeds, when properly moiſtened; for they will all vegetate freely in open ſituations; but the following experiment ſeems to prove that trees, ſhrubs and the leſs juicy herbs derive air from water, as fiſhes do.

Exp. 7. I introduced the lower extremities of different plants, or more properly of the branches of different plants, into phials filled with water, and after luting the necks with wax, I ſunk the bottles in pots of water, ſo as to leave the foliage of the plants expoſed to the ſun and ſurrounded by the atmoſphere. The evaporation cauſed by the action of the light drew the water out of the bottles, and the air deſcending at the ſame time through the ſubſtance of the plants prevented a vacuum being formed in theſe cloſe veſſels. This fact had nearly perſuaded me, that vegetables poſſeſs a faculty analogous to reſpiration; but when I repeated the experiment with phials perforated in the bottom, they remained full of water. This circumſtance ſhews the reſpiration in queſtion to be merely accidental and to be occaſioned by the necks of the bottles being luted; on the other hand it is highly probable, that air enters the veſſels of vegetables in conjunction with water. Here it is decompound, in conſequence of the ſtimulus given to the vegetative principle by its preſence; the azote is afterwards aſſimulated into the ſubſtance of the plant, while the oxigen is rejected either wholly or in part. This gas, after its expulſion flows into the mephitic air, contained in the jars, the volume of which is thereby augmented, and its quality improved; but I will not venture to ſay, that the two gaſes form a new chemical compound on theſe occaſions.

Exception to the general rule that plants require air.

Exp. 8. I am acquainted with a ſingle exception to the general rule that air is requiſite in the proceſs of vegetation. For when plants have withered in azote they become mouldy; that is, in the language of botany, they are covered with the mucor mucedo. The worms which inhabit the decayed livers of ſheep, &c. furniſh us with a parallel inſtance in the animal kingdom; and the inveſtigation of theſe ſingular anomalies forms a difficult problem in the natural hiſtory of organized bodies.

The eſſay concluded.

The ſentiments contained in this letter, are oppoſite to the common notions relative to the nature of vegetation, and ſo manifeſt a departure from eſtabliſhed opinions may be thought to require an apology. The juſtification of my conduct however appears in the body of the eſſay. The experiments are related with an attention to circumſtances; which will enable almoſt any perſon to repeat them; and ſhould ſome philoſopher undertake to do it, his labours will either eſtabliſh my opinions, or confirm the contrary doctrine, by the refutation of them. There is one omiſſion in the detail of theſe experiments; I have neglected to remark, that the term azote ſignifies nothing more than common air deprived of oxigen by the ſulphuret of potaſh or the martial paſte of ſulphur, but more frequently by the former. The determination of the queſtion is of ſome moment, not only to natural hiſtory, but perhaps to agriculture alſo; and I could wiſh it to be reſumed by a philoſopher, whoſe advantages are ſuperior to my contracted powers of obſervation and imperfect apparatus. Should any man of ſcience undertake the taſk, I would recommend the ſedum acre to his notice; becauſe the filaments which ſpring from the ſtalk of this plant in common air afford a criterion whereby the progreſs of ſimilar ſpecimens confined in azote may be obſerved and aſcertained.

JOHN GOUGH.

Middleſhaw,
October 24, 1804.

SCIENTIFIC NEWS, ACCOUNT OF BOOKS, &c.

Frothing of Oil by Electricity.

THE anonymous author of ſome electrical experiments communicated to Van Mons the following fact, from whoſe Journal I tranſlate it.[*] He was making an experiment to aſcertain the conducting quality of oil, for which purpoſe he had filled one third part of the length of a tube with the oil of Colſa. The tube was three feet long and half an inch in diameter, having its cloſed end drawn out: The open extremity was ſecured by a cork, through which a needle was thruſt; and over the cork was a coating of wax to prevent the oil from tranſuding. The tube being then placed on two inſulating ſupports, and the needle turned towards the part moſt remote from the machine, the tube was ſlightly inclined, in order that the oil might run down to that part of its capacity which was neareſt the prime conductor. He then put the machine in action, and with great ſurpriſe obſerved the oil gradually to enter into a ſtate of ebullition; beginning at the ſurface and ſpeedily acquiring a ſtate of effervescence perfectly reſembling that of ſparkling Champaigne. Upon opening the tube, a gas eſcaped with violence, which the author did not collect, but which well deſerves to be examined. This active electrician intends to ſubject the different vegetable and mineral acids to the action of electricity by the ſame proceſs.

Native Magneſia.

GIOBERT has found that a white earth conſidered as pure alumine and employed at Turin in the fabrication of porcelain, contains 0,80 of magneſia. VAN MONS.

Gray's Experienced Mill-wright.

The Experienced Mill-wright; or a Treatiſe on the Conſtruction of ſome of the moſt uſeful Machines, with the lateſt Improvements. To which is prefixed a ſhort Account of the general Principles of Mechanics and of the Mechanical Powers. Illuſtrated with forty Engravings. By Andrew Gray, *Millwright, very large quarto, 73 Pages of Letter Preſs, of the ſame Size as the Engravings.*

THE author is a practical mechanic, who has been upwards of forty years employed in erecting different kinds of machinery; and the greateſt part of the Deſcriptions in this Work are of Machines which he has either planned or ſuperintended their conſtruction. Theſe Drawings being accurately made to ſcale, are not, as he remarks, to be conſidered as mechanical ſpeculations, always doubtful till ſubjected to actual experiment, but as caſes of practical knowledge, the effects of which have been fully tried and long approved; moſt of them being ſtill employed for the purpoſes of their original fabrication.

Previous to his Account of the Machines, this author gives a popular Treatiſe on Mechanics, in ſix different chapters, beginning with General Principles and Definitions, which are followed by Explanations of Simple Engines, called the Mechanical Powers. Theſe are ſucceeded by an Account of the Effects of Friction, and the beſt Method of applying thoſe Mechanical Powers. Practical Directions for deſigning and conſtructing the Parts of Machines are in the next place given; and laſtly, the author treats on the Strength of Machines, the relative Velocity of their Parts and the leading Doctrines which are neceſſary to be attended to in the erecting of Water-Mills.

The Machines of which Meaſured Plans, Elevations and Sections are given, are; A conſiderable number of Thraſhing Machines, to be drawn either by Water or by Horſes, with the Elevation of a Wind-mill to turn Machinery of this Deſcription; one Snuff Mill; two Sheeling Mills; ſeveral Corn, Barley, Malt, and Flour Mills; one Oil Mill; one Flax Mill; one Paper Mill; one Saw Mill; one Fulling Mill; Bleaching Machines, Beetling Machines, and various Pumps.

Theſe Drawings and Deſcriptions will no doubt prove of high value to the man of buſineſs; though it is perhaps to be regretted that the ſame attention to junior ſtudents which is diſplayed in the introductory treatiſe, is not as fully ſhewn by explaining the relative operations and effects of the parts of theſe machines in the deſcriptions which accompany the drawings.

Practical Obſervations concerning Sea Bathing; to which are added, Remarks on the Uſe of the Warm Bath. By A. P. Buchan, *M. D. of the Royal College of Phyſicians. London. Crown 8vo. 207 Pages.*

Buchan on ſea bathing.

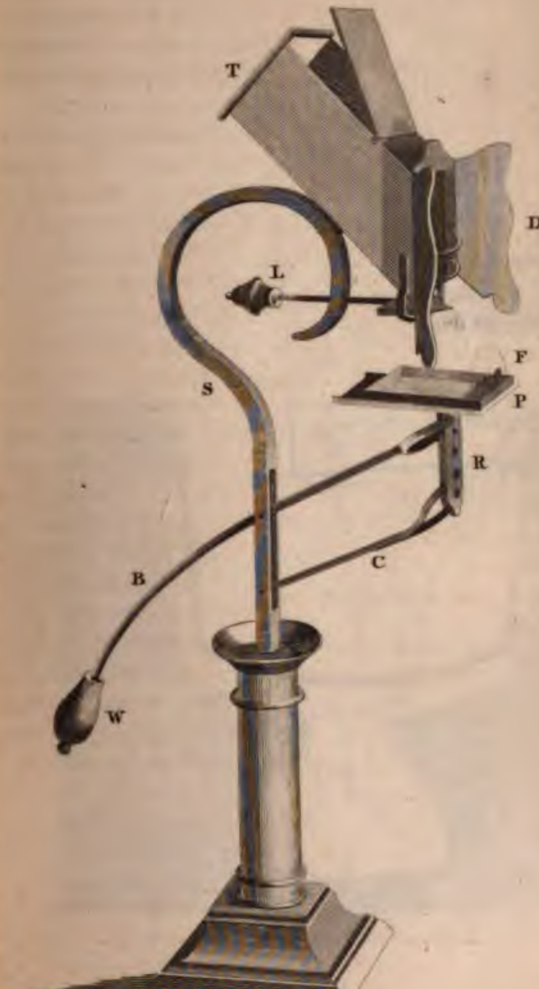
IN the early ſtages of human ſociety, when the occupations of man led him to ſtrong exerciſe in the open air, it is probable that his health may have been chiefly impaired by the fortuitous occurrences of his mode of life, and the direct wear of his organs engaged in the groſs and active labours of his life. It is probable that he may have felt little of thoſe diſeaſes which the claſſification and habits of a refined and luxurious ſociety have introduced. Neither mental fatigue, nor the anxious and inceſſant labour of crowded cities could have operated upon his powers, nor was he ſubjected to the miſeries of indolence, repletion, and the liſtleſs want of motive which benumb the faculties of the rich. Theſe are the prices which

Buchan on ſea bathing.

by our numerous miſtakes we are compelled to pay for the ſuperior enjoyments which an enlightened ſtate of ſociety can afford to thoſe only who poſſeſs the virtue, the conduct, and the good fortune to know and to acquire them. Among the moſt oppreſſive of theſe conſequences, no doubt, is the loſs of health; and while we make ſacrifices to what we imagine to be our welfare, it becomes us occaſionally to interrupt our uſual train of habits, to unbend the mind, give repoſe to the ſyſtem, and recover, by an occaſional change of our occupations, that vigour which ſameneſs of purſuits and confinement ſeldom fail to impair.

One of the means at preſent much reſorted to is, to repair to the ſea-coaſt; partly with a view to enjoy a ſcene which, by its impreſſive magnitude and the numerous changes it undergoes, cannot but be highly intereſting; but principally with the intention of bathing in the ſea. Bathing is certainly of the greateſt benefit in many caſes, and, like every other practice capable of ſtrongly affecting the human frame, it muſt alſo, in other caſes be capable of operating to the injury of the health. We have no direct general treatiſe on this ſubject; and to give the requiſite information to individuals by which they may ſafely and judiciouſly regulate themſelves requires ſomewhat more than mere medical ſkill. The preſent work abounds with intereſting facts and clear deductions, and is written with that candid ſpirit of philoſophical remark which ſhews an intimate acquaintance with the moral as well as the phyſical conditions of our exiſtence, and the accidents to which we are expoſed. I am much miſtaken if any one who begins to read this work will be diſpoſed to quit it without peruſing the whole; and there are few who can have ſo extenſively conſidered the ſubject, or who are ſo little concerned in its diſcuſſion as not to derive advantage from the many points of information it contains.

Tallow Lamp by Mr. J. W. Boswell.



by Blunt.
Engraved by Mutlow.

THE N
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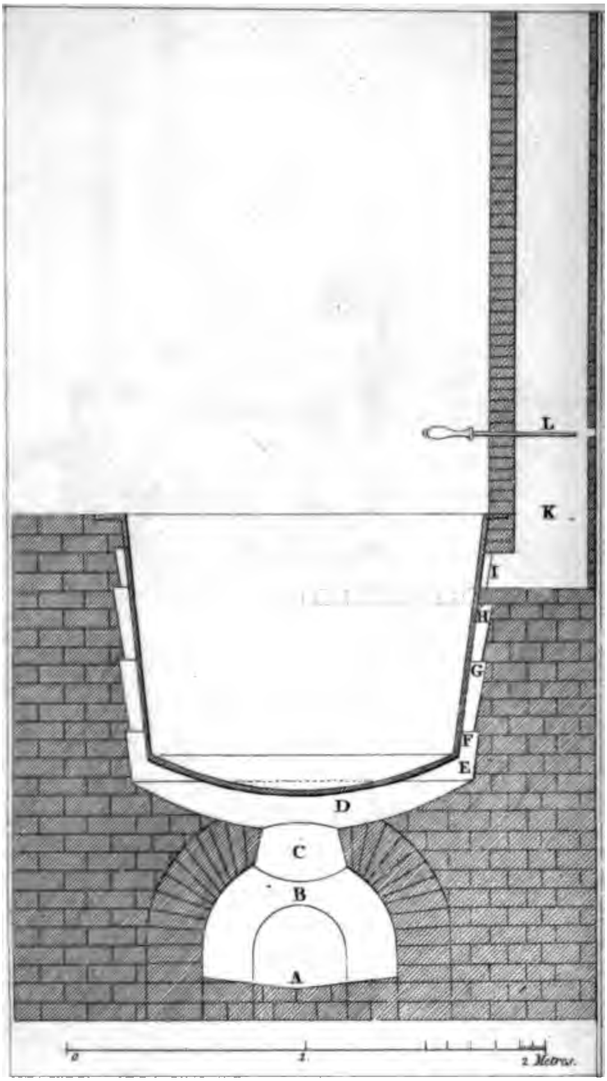
Evaporating Furnace by Curaudau.



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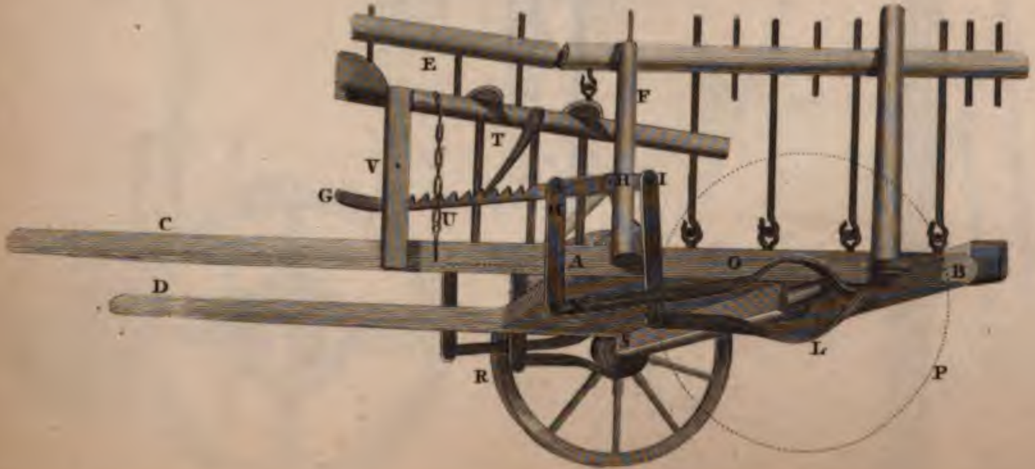
Mr. Wm. Bowler's double Brace for Carriages.



Mallow Sc. Rufsell Ct.

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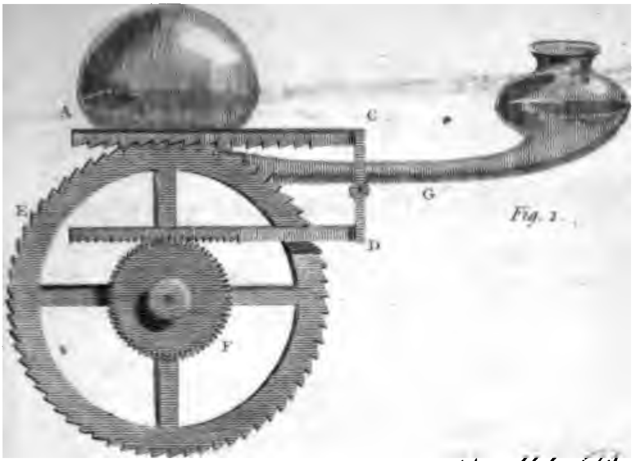
Perpetual Motion.



Parallel Ruler.

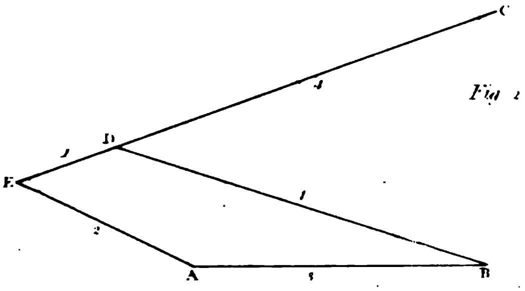
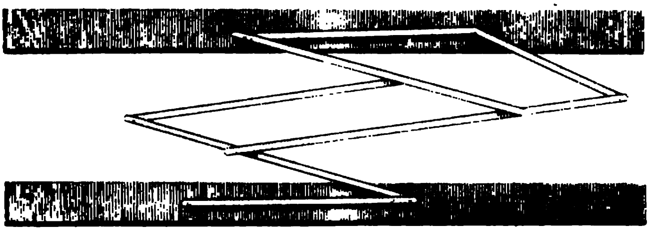


Fig. 3.



A
JOURNAL
OF
NATURAL PHILOSOPHY, CHEMISTRY,
AND
THE ARTS.

DECEMBER, 1804.

ARTICLE I.

Deſcription of a Tubular Pendulum; having all the Properties of the Gridiron; but being more compact as well as more ſteady in its Motions. In a Letter from Mr. EDWARD TROUGHTON, *the Inventor.*

To Mr. NICHOLSON.

DEAR SIR,

Advantages of publiſhing improvements in philoſophical apparatus.

EVER ſince the commencement of your Journal, I have often thought of regiſtering therein, the improvements which I frequently make in aſtronomical and other inſtruments. No man perhaps had ever a greater antipathy to writing than myſelf, and drawn as I am into the vortex of buſineſs, and working with my own hands from nine to twelve hours in a day, leaves me but little time for literary purſuits; yet in one who values himſelf on account of the originality of his ideas, it muſt ſeem ſlothful not to commit them to the preſs, which until that is done, are like flowers or fruit growing fenceleſs by the way ſide, which every one that paſſes by may gather.

Pendulums for clocks are particularly ſuited to mathematical workmen.

I always conſidered the pendulum of a clock more ſuitable to our ſtile of workmanſhip and habits of thinking, than to thoſe of any other deſcription of artiſts; and agreeable to this idea, I have made pendulums near twenty years, and I own the ſubject is ſtill agreeable to me, being yet unincumbered with patents, the reaſon of which no doubt is, that the few wanted holds out no proſpect of gain.

Tubular pendulum.

Under theſe circumſtances, I offer you the following deſcription of my tubular pendulum, which I contrived about the middle of laſt July, and which I have juſt brought into action. The honourable mention which you made of my mercurial pendulum (Phil. Journal for May 1797) the excellent papers which you have at different times given to the public on this ſubject, and its importance to practical ſcience, eſpecially aſtronomy, leave me no room to doubt but this communication will be agreeable to you.

It acts on the principle of the gridiron.

It may here be proper to remark that the nature of this pendulum is by no means new; it is of the gridiron kind, and although extremely unlike it in outward appearance, is only a new ſpecies of that genus. Without further introduction, I will now proceed to give a brief deſcription, with reference to the accompanying ſketches, and then conclude with a few general remarks.

Deſcription of the tubular pendulum by the drawing.

Fig. 1. and 2, *Plate* XIII. drawn to a ſcale of one eighth of the real dimenſions, exhibit the ſhape of the whole inſtrument, in which the parts of action being completely concealed from view, it appears, excepting the uſual ſuſpenſion ſpring, to be made of ſolid braſs. The firſt of theſe figures gives a front view, the latter ſhews it as ſeen edgewiſe. This figure of the bob is uſed more on account of its being eaſy to make, and ſightly, than from any other conſiderations; it is made of one piece of braſs, about 7 inches diameter, 2,5 thick at the centre, and weighs about 15 pounds avoirdupoiſe: the front and back ſurfaces are ſpherical, with a thick edge or cylindrical part between them. The apparent rod is a tube of braſs, ſhewn in both figures to reach from the bob nearly to the top; this contains another tube and five wires in its belly, ſo diſpoſed as to produce altogether (like the nine-bar gridiron of Harriſon) three expanſions of ſteel downwards, and two of braſs upwards; whoſe lengths being inverſely proportioned to their dilatation, when properly combined, deſtroy the whole effect that either metal would have ſingly. The ſmall viſible part of the rod near the top, is a braſs tube, whoſe uſe is to cover the upper end of the middle wire, which is here ſingle, and otherwiſe unſupported.

Description of the tubular pendulum by the drawing.

Reckoning from the top the firſt action is downwards, and conſiſts of the ſpring, a ſhort wire 0,2 diameter, and a long wire 0,1 diameter; theſe all of ſteel firmly connected, reach down within an inch of the centre of the bob, and occupy the middle line of the whole apparatus. To the lower end of the middle branch, is faſtened the lower end of the interior braſs tube, 0,6 in diameter, which terminates a little ſhort of the top of the exterior tube, and produces the firſt dilatation upwards. From the top of the interior tube depend two wires 0,1 diameter, whoſe ſituation is in a line at right-angles to the ſwing of the pendulum, and reach ſomewhat lower than the attached tube itſelf, which they paſs through without touching, and effect the ſecond expanſion downwards. The ſecond action upwards is gained by the exterior tube, whoſe internal diameter juſt allows the interior tube to paſs freely through it: its bottom is connected with the lower ends of the laſt deſcribed wires. To complete the correction, a ſecond pair of wires of the ſame diameter as the former, and occupying a poſition at right angles to them, act downwards, reaching a little below the exterior tube, having alſo paſſed through the interior one without touching either. The lower ends of theſe wires are faſtened to a ſhort cylindrical piece of braſs, of the ſame diameter as the exterior tube, to which the bob is ſuſpended by its centre.

Fig. 3. is a full ſize ſection of the rod, in which the three concentric circles are deſigned to repreſent the two tubes, and the rectangular poſition of the two pair of wires round the middle one, are ſhewn by the five ſmall circles. By copying this arrangement, from the elegant conſtruction of your own half ſeconds pendulum (Philſ. Journal for Auguſt, 1799) I avoided much trouble, which muſt have occurred to me, unleſs indeed, I had been impelled on the ſame idea, by the difficulty of contriving the five wires to act all in a row, with ſufficient freedom and in ſo ſmall a ſpace. *Fig.* 4. explains the part which cloſes the upper end of the interior tube: the two ſmall circles are the two wires which depend from it, and the three large circles ſhew the holes in it, through which the middle and other pair of wires paſs.

Fig. 5. is deſigned to explain the part which ſtops up the bottom of the interior tube, the ſmall circle in the centre is where the middle wire is faſtened to it, the others the holes for the other four wires to paſs through. *Fig.* 6. is the part which cloſes

Deſcription of the tubular pendulum by the drawing.

cloſes the upper end of the external tube, the large circle in the centre is the place where the braſs covering for the upper part of the middle wire is inſerted, and the two ſmall circles denote the faſtening for the wires of the laſt expanſion. *Fig.* 7 repreſents the bottom of the exterior tube, in which the ſmall circles ſhew the faſtening places for the wires of the ſecond expanſion, and the larger ones the holes for the other pair of wires to paſs through. *Fig.* 8. is a cylindrical piece of braſs, which ſhews how the lower ends of the wires of the laſt expanſion are faſtened to it, and the hole in the middle is that whereby it is pinned to the centre of the bob. The faſtening of the upper ends of the two pair of wires is done by ſcrewing them into the pieces which ſtop up the ends of the tubes, but at the lower ends, they are all fixed as repreſented in *Fig.* 8. I have only to add to this deſcription, that the pieces repreſented by *Fig.* 7 and 8, have each a jointed motion, by means of which the fellow wires of each pair would be equally ſtretched, although they were not exactly of the ſame length.

In the apparatus thus connected, the middle wire will be ſtretched by the weight of the whole; the interior tube will ſupport at its top the whole except the middle wire: the ſecond pair of wires will be ſtretched by all except the middle wire and interior tube; the exterior tube ſupports at its top the weight of the ſecond pair of wires and the bob, and the ſecond pair of wires are ſtretched by the weight of the bob only.

The firſt pendulum which I made of the tubular kind, had only three ſteel wires, and one tube above the bob; that is two expanſions down and one up; and the quantity which one of braſs falls ſhort to correct two of ſteel, was compenſated for, by extending thoſe branches of the rod below the bob, and bringing up an external tube to which the bob was affixed. There is an aukwardneſs in this conſtruction, owing to the rod reaching about 13 inches below the lower edge of the bob, otherwiſe, it is not inferior to the one firſt deſcribed.

Difficulty of preventing flexure and ſtarting motion in the gridiron,

The principles of the gridiron pendulum, I believe, have never been queſtioned, indeed they cannot be; the difficulty of conſtructing it ſtrong enough to prevent lateral flexure in the lifting bars, and conſequent friction in the holes of the different croſſing pieces neceſſary to bind it together, which occaſion it to act by ſtarts, has been the only obſtacle to prevent its general application to the beſt clocks.

To remedy this evil I contrived the one here treated of, and in my own judgment, have ſucceeded as near perfection as any effort of mine ever did. The tubes are drawn ſtraight and regular in an engine, in the manner that wire is done: and their ſtrength aſſure ſucceſs, preventing the poſſibility of any ſenſible bending, while the freedom of action prevents any irregular motion whatever.

—is prevented by the uſe of tubes.

I have no farther quarreled with my own conſtruction of the mercurial pendulum, the principles of which are faultleſs, and form elegant; than that it is quite unfit for carriage, and an article too hazardous for me to deal in; but after all, candour obliges me to give the preference to the original one of Graham, which on account of ſome ſenſeleſs criticiſms about one part taking heat ſooner than another, evaporation of the mercury, &c. was too haſtily laid aſide. Graham undoubtedly made his pendulum before the expanſions of either braſs or mercury were accurately known; but an ingenious friend of mine, has from the beſt experiments, computed the different parts to mathematical preciſion, and introduced it a ſecond time to the world; and already, there are not wanting proofs of its practical excellence.

Troughton's mercurial pendulum is perfect but not portable.

Graham's pendulum approved.

Smeaton's pendulum with tubular bob is a good one, but on account of the glaſs rod, like my mercurial one, is totally unfit for tranſportation, and like Graham's, is unſightly; while all three labour under the unavoidable diſadvantage of not being applicable to the pyrometer in their compound and pendant ſtate.

Smeaton's good; but not portable.

I am about to conſtruct a pyrometer fit to try the tubular pendulum in its finiſhed ſtate; a pin fixed in the centre of oſcillation, ſhall be contrived to act on a nice ſpirit level, and ſhew the expanſion, if any, in angular meaſure: by which I ſhall be enabled to adjuſt it; for I have no idea of adjuſting one by the going of a clock, that complicated apparatus will always, I fear, have too many errors of its own, to be uſed as a criterion of the merits of this more perfect inſtrument.

I will here ſubjoin the lengths of the different parts of tubular pendulums in inches; alſo, the expanſions of braſs and ſteel from which I computed them. The length of the interior tube is 31,9, and that of the exterior one 32,8, to which muſt be added 0,4 the quantity which in this pendulum, the centre of oſcillation is higher than the centre of the bob; theſe all

Dimenſions and grounds of conſtruction of the tubular pendulum.

all of braſs. The parts which are of ſteel are the middle wire, including 0,6 the length of the ſuſpenſion ſpring, is 39,3, firſt pair of wires 32,5, and the ſecond pair 33,2. From experiments which I tried in the year 1794, I found my braſs to expand with 60° of heat 0,000640 on an inch, and my ſteel 0,0003966. Smeaton obtained under the ſame circumſtances 0,0006444 for braſs, and 0,0003833 for ſteel. Roy found braſs to expand 0,0006316 and ſteel 0,0003816.

Expanſions of braſs and ſteel.

The above lengths computed from my expanſions will I think be found very correct; if they are tried by Smeaton's, the pendulum will ſeem to err in exceſs, or gain with heat, about 0″,9 a day with 60° of Fahrenheit; if Roy's be uſed, it will err alſo in exceſs, under the ſame circumſtances ſomething more than one ſecond. The above ſtatement ſhews with what care experiments of this kind ought to be conducted. All the dimenſions are given in inches and decimals.

If my pyrometrical experiments turn out ſucceſsful, they may likely be the ſubject of another communication; and I will reſerve till then my reaſons for giving the preference to my own expanſions in the conſtruction of the inſtrument here deſcribed; for this letter, being twice as long as I firſt intended, ſhall now be concluded by,

DEAR SIR,

Your obliged and obedient Servant,

EDWARD TROUGHTON.

November 3, 1804.

II.

Letter from W. JESSOP, *Eſq. on an Improvement in the Proceſs of blaſting Rocks with Gun-powder.*

To Mr. NICHOLSON.

SIR,

IF I am communicating to you what you already know, you will accept as an apology for it that it is not generally known, and will be intereſting to all who are concerned in mining or blaſting rocks with gunpowder.

Uſual proceſs of boring and blaſting,

The uſual proceſs, after drilling a hole and charging it with powder, is to introduce a wire or ſmall iron rod, to preſerve a communication with the fuze, and then to ram up the remainder of the hole with ſtone pulverized by the operation of ramming it; after which the wire is withdrawn and the priming introduced.

is tedious, dangerous and uncertain.

This is a tedious operation, often attended with danger, and frequently the labour and powder is loſt by the priming hole being obſtructed in ſtriking out the wire with a hammer.

Looſe ſand propoſed inſtead of ramming.

I had been informed that inſtead of this tedious operation, the blaſting had been effected by introducing a ſtraw filled with fine powder, and then filling the hole with looſe ſand.

I hardly could believe this, conceiving that the part where there would be the leaſt reſiſtence would firſt yield to the exploſion, and therefore that the looſe ſand muſt of courſe be blown out.

Succeſs of the experiment.

But in the month of Auguſt laſt I tried the experiment in ſome very hard rock at Fortwilliam and it completely ſucceeded: I ſince then have tried it on the lime rocks at Briſtol with the ſame effect.

Oak 20 inches diameter ſplit in pieces under a charge of 3 inches ſand.

A few days ſince, wiſhing to know how ſmall a quantity of ſand would produce the effect, I cauſed a hole of 1½ inch in diameter and 12 inches deep, to be bored in a knotty piece of oak of about 20 inches in diameter and charged it with three inches of powder, and upon that four inches of ſand, intending (as I ſuppoſed that muſt be blown out) to repeat the quantity of ſand inch by inch to the quantity that would be ſufficient, but to my great ſurprize, it ſplit the piece with great violence into ſix pieces.

The ſame effect with leſs powder.

I then repeated it on a ſimilar log of oak, with a charge of two inches of powder, and *three* inches of ſand; it ſplit the piece in two, and ſent half of it into the air to the diſtance of forty yards.

Conjecture as to the cauſe.

I think it may puzzle a philoſopher to account for this: I have ſuppoſed that the ſand in contact with the powder, firſt moving, the particles wedge each other faſt before the motion has time to be communicated to the whole.

Leſs powder will probably be required in this way.

I have reaſon for thinking that a much leſſer quantity of powder will produce the effect in this way than by the common method; for as it lies looſe in its chamber and makes itſelf more room by a little yielding of the ſand, the whole will take fire, and eſpecially if when half the charge is put in, the ſtraw be introduced and then the other half charge, ſo that it may take fire in the middle of the charge.

That

The ſand appears to yield a little.

That the ſand will yield a little, I think I may infer from having charged a tube of tin 14 inches in length and 1¼ diameter, with three inches of powder; on exploding it immerſed in water, I found *five inches* of the tube abruptly cut off, while the remainder was uninjured, but I do not conſider this inference as deciſive.

For want of time I was prevented from trying how much *leſs* than three inches of ſand would have been ſufficient.

I am, Sir,
Your moſt obedient,
W. JESSOP.

London Coffee Houſe,
Nov. 8, 1804.

A charge of ſand would probably deſtroy artillery.

P. S. Might not this be uſeful on the evacuation of a fort, in deſtroying cannon, inſtead of the ineffectual mode by ſpiking?

III.

On the Mucilaginous Matter of certain Vegetables, and their Uſe as a Subſtitute for Gum Arabic: by Mr. THOMAS WILLIS; *being a Continuation of Experiments made upon the Subject by him, in Addition to thoſe formerly publiſhed in the Tranſactions of the Society of Arts.**

Letter to CHARLES TAYLOR, *Eſq.*

SIR,

Mucilaginous powders from vegetable roots.

I HAVE taken the liberty of preſenting to the Society for the Encouragement of Arts, &c. ſpecimens of the powder of vernal ſquills, of white lily-roots, and of ſalop, for their conſideration and trial. I believe the bulbs of the vernal ſquill will be found to be equally uſeful as thoſe of the hyacinthus non-ſcriptus; and the ſtrong mucilage from the white lily-root and the ſalop much more ſo.

As gum-arabic has been at times, during the laſt war, above 30*l.* per cwt. the multiplication of different ſubſtances that will anſwer the ſame purpoſe, will aſſuredly be an object of a very intereſting nature to manufacturers in time of war.

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* Extracted from their Tranſactions, Vol. XXI. The former experiments are in our Journal, VII. 30.

I have ſent all the powder of white lily-root I had left; but am at the preſent time drying ſome taken from the garden two or three days ago. As ſoon as it is powdered, I will ſend you a large quantity of it.

I am, Sir,
Your moſt obedient Servant,
THOMAS WILLIS.

March 24, 1803.

SIR,

FROM the candour and approbation my paper on the uſe of the hyacinthus non-ſcriptus, as a ſubſtitute for gum-arabic, in ſome trials in calico-printing, met with from the Society for the Encouragement of Arts, &c. I reſolved to purſue the ſubject further; and now offer the following obſervations for your conſideration, in hope that trials may be made, that will be found to be of general benefit.

Root of the vernal ſquill very mucilaginous.

Having frequently obſerved, in old gardens, that the vernal ſquill grew very prolific, I conceived the root of them might be equally as mucilaginous as the roots of the blue bells. I procured three pounds ſix ounces of them on the 8th of July, 1802, and ſliced and dried them. They produced one pound one ounce of powder, one drachm of which was diſſolved in four ounces of water, by letting the mixture boil a minute or two. When the liquor was cold, there was a mucilage full as ſtrong as one made with a drachm of the powder of blue bells, in the ſame proportion of water; from which circumſtance, I ſhould think the vernal ſquill will anſwer the ſame purpoſes as the hyacinthus non-ſcriptus. If it ſhould, it is a root that may be very eaſily and plentifully propagated, and whenever gum-arabic is dear, may be found uſeful. It is to be remarked, that I found no pungency in the powder of the vernal ſquill. I have frequently eaten of it, and the taſte is rather agreeable.

White lily root alſo very mucilaginous.

On the 18th of Auguſt, 1802, I collected four pounds of the white lily root, which yielded, when dried, rather more than one pound of powder. A drachm of it was diſſolved in four ounces of water, by gently boiling it a minute or two: the mucilage was much ſtronger than that made with the vernal ſquill, but ſomewhat darker coloured. This root may poſſibly anſwer the ſame purpoſes better than thoſe above mentioned. The powder of the lily-root leaves a bitterneſs on the tongue.

Gum obtained by ſolution and evaporation.

Twelve ounces of the freſh roots of the white lily being bruiſed and preſſed, yielded by evaporation one ounce and one quarter of brown gum.

There being a ſmall quantity of it, I made no trial with it; but very probably the expreſſed juice might be uſed by the calico-printers with advantage. Theſe roots can be procured at all times, and propagated to any extent; but it muſt be obſerved, that theſe bulbous roots are ſtronger when they are without ſtems, or only beginning to ſhoot out leaves; and the preſent time is as proper as any that can be for procuring them.

Comfrey root.

I tried comfrey-root; but the dark colour of the cuticle of the root affected the ſolution, by making it of a dark dirty brown.

I do not pretend to claim any merit in making the above experiments. Every one who is acquainted with theſe roots, well knows that they are all mucilaginous. My deſign is only your patronage, to render them uſeful in the arts, that their virtues and effects might not lay dormant, but be rendered ſerviceable to trade.

Powder of ſalop root affords very ſtrong mucilage; probably cheaper than gum-arabic.

After making the above trials, the powder of ſalop-root was uſed, by diſſolving one drachm of it in four ounces of water, in the manner above mentioned. It produced a very ſtrong mucilage, and, when cold, was a perfect jelly, and much clearer than either of the other ſolutions. I am greatly of opinion, that the powder of this root will not only anſwer all the purpoſes of gum-arabic, but will be found full as cheap, if not cheaper, in proportion to its ſtrength, than gum-arabic; but this is ſubmitted to your conſideration and trial.

I have ſent ſome ſpecimens of the Powder of the Vernal Squill, of White Lily Root, and alſo of the Salop Powder, in order that they may be tried by your direction; and if they ſhould be found of benefit to commerce, nothing would give me greater pleaſure, than to find my ſlender abilities have been of utility to my country, and honoured with the approbation of the Society.

I am, Sir,
Your obedient ſervant,
THOMAS WILLIS.

March 22, 1803.

Examination

IV.

Examination of Mr. Ezekiel Walker's Experiments and Theory of the Enlargement of the Horizontal Moon. In a Letter from C. L.

To Mr. NICHOLSON.

SIR,

Aſſertion of Mr. Walker that optical images vary with the pupil.

YOUR correſpondent Mr. Ezekiel Walker, has favoured the world with a new attempt to account for the apparent enlargement of the horizontal moon *, in which, after indulging his wit, and expreſſing his aſtoniſhment at the expence of thoſe who have laboured on the ſame ſubject, he proceeds to aſſure us that the *pictures on the retina are not permanent but vary as the dimenſions of the pupil vary.*

That you, Mr. Nicholſon, who are in ſome reſpect accountable to the world for what you admit into your Journal, and who poſſeſs a juſt celebrity for the manner in which you have executed the taſk,—that you ſhould have omitted to favour the writer and your readers ſo far as to reject his paper, is to me no otherwiſe to be explained than by referring it to your impartiality; which may have induced you to leave the refutation of alledged facts to your correſpondents rather than enter into a diſcuſſion reſpecting them yourſelf.

Conſequences that would follow if it were well founded.

I cannot for a moment ſuppoſe you to have had a doubt concerning this imaginary novelty in optics. If Mr. Walker's poſition were true, the magnifying powers of the ſame teleſcope would vary with its aperture; a well illuminated theatre or room would become contracted in its apparent dimenſions; the paper at which I now look would ſuddenly become larger when I cover the candle with my hand; and in a word we ſhould have no certainty if the magnitudes of things were to appear different with every change of the pupil.

Experiments of Mr. Walker repeated.

None of theſe things do in fact happen, and it ſeems almoſt trifling to inſiſt upon them. Still leſs is it neceſſary to have recourſe to lines and figures: but for the ſake of your leſs inſtructed readers I will meet Mr. Walker by correct experiments of the ſame deſcription as his own.

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* Philoſ. Journal, IX. 164.

Exp.

The image formed by a lens with a broad aperture is the ſame as

Exp. 1. I took a clear double convex lens ſupported on a ſtand, and placed the ſame oppoſite an Argand's lamp; ſo that a good image of the flame was formed upon a paper fixed againſt the wall of the room. From the flame to the lens the diſtance was 49½ inches and from the lens to the image it was 88 inches. The horizontal breadth of the image was repeatedly meaſured = 1.58 inches and to a greater degree of preciſion than one hundredth of an inch.

with a contracted aperture.

Exp. 2. The lamp and lens were ſuffered to remain unaltered in the ſituations they poſſeſſed in the former experiment; but a ſcreen was placed cloſe to the lens, having a circular hole of one inch diameter concentric with the lens itſelf. The image was then fainter but very diſtinct, and meaſured exactly the ſame as before.

In both theſe experiments the papers on the wall were ſeveral times changed to prevent any deception from the appearance of former meaſures; and the meaſures were taken along the ſame horizontal line by the help of a line on the wall.

Hence it was ſeen that a diminution of the pupil or aperture, to admit leſs than one thirty-eighth part of the light, made no difference in the image.

The experiments repeated with an achromatic lens.

Exp. 3. I took a good achromatic object glaſs of Dollond, having an aperture of 1.9 inch and placed it at the diſtance of 52½ inches from the lamp; when it gave a good image on the wall at the diſtance of 70¼ inches from the lens. The horizontal breadth of the image was 1.2 inches.

Exp. 4. When the experiment was repeated with no other change than that a ſcreen with a central hole of 0.3 inches was placed before the lens, the image proved 1.2 inches as before.

The two laſt experiments were made with the ſame care and precautions as the former. They ſhew that no diminution of the image takes place when the pupil is diminiſhed to admit leſs than one fortieth part of the light.

Probability that Mr. W. was deceived, and how.

I may not conclude, Sir, without making ſome obſervations on the direct contradiction of my experimental reſults to thoſe of Mr. Walker's. May I aſcribe to him any thing more than inaccuracy? Aſſuredly I do not. Nevertheleſs, if he meaſured ſo uncertain a thing as *the length* of the flame of a candle without being aſſured by repeated experiments that its permanency was entitled to ſome confidence; if he held his ſcreen nearer to his lens when the light was rendered fainter by his card; if he miſtook the greater viſibility of the ſmaller image for a proof that it was in the focus of the lens; in a word, if he did not pauſe and examine former facts before he adopted a concluſion ſo repugnant to many of them, I would ſubmit to yourſelf and readers whether he has acted conſiſtently with the rules of philoſophical inveſtigation, or has any reaſon to prize his own ſolution beyond that which is founded on the rules of linear perſpective*.

I am, Sir,
Your humble ſervant,
C. L.

V.

The Method of preparing Chineſe Soy. *By* MICHAEL DE GRUBBENS.†

Ekeberg's account of ſoy is erroneous.

IN the acts of the Swediſh Academy, for 1764, page 38, we find a deſcription of the preparation of Chineſe Soy, by the late Captain Ekeberg; but as this deſcription is not only incomplete but even deficient in accuracy, ſo as not to produce the true Chineſe compound, I am well convinced that Mr. Ekeberg muſt have been unacquainted with the actual proceſs. It is moſt likely that he depended upon information given him by the Chineſe who are not always to be truſted; a fact of which I have had ſufficient reaſons to be convinced, during my five years reſidence in their country, in my attempts to aſcertain correctly the manner of treating a ſpecies of ſilk-worm which affords its thread five or ſix times in the year, as well as their methods of dying cotton and ſilk and ſeveral other branches of domeſtic œconomy.

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* Mr. W. having ſuppoſed that the want of land objects muſt take away the notion of diſtance at ſea, reminds me of an incident concerning the Panorama of Black-Friar's Bridge. This picture was exhibited with indications of a conſiderable wind with waves on the river, at the ſame time that the buildings on the London ſide were ſeen reflected in the water. I took notice of this inconſiſtency, and the ingenious author replied that the water had been at firſt painted as if ſmooth; but it was found neceſſary to put in the waves in order to give diſtance by their regular diminution in perſpective, and that the reflections were kept in from a notion that they would rather add to the effect than offend by their want of perfect truth.

† Extracted from the Memoirs of the Academy of Sciences of Stockholm for 1803, by M. Linborn, and inſerted in the Annales de Chimie, Vol. L. from which the preſent tranſlation is made.

Having ſince received for a conſiderable remuneration true notions concerning theſe objects, I ſaw how far the former recitals had differed from the truth. The ſame thing happened in my attempts to diſcover the preparation of ſoy; but as I at laſt became perfectly acquainted with it, I think it proper to communicate the ſame to the academy.

Ingredients of ſoy.

Soy is prepared with a ſpecies of haricots (which are white and ſmaller than thoſe of Turkey) wheat flour, common ſalt, and water. The following are the proportions; 50 pounds of haricots, 50 pounds of ſalt, 60 pounds of wheat flour, and 250 pounds of water.

Proceſs. Haricots are boiled in water,

—then mixed with wheat flour,

After having well waſhed the haricots, they are boiled in well-water in an open veſſel for ſome hours, or until they have become ſo ſoft as to be worked between the fingers. During the boiling they muſt be kept covered with water to prevent their burning, and care muſt be taken not to boil them too much, becauſe in that caſe too much of their ſubſtance would remain in the water of decoction. The haricots being thus boiled, are taken out, and put into large ſhallow wooden veſſels, which in China are made of thin ſtaves of bamboo, two inches and a half in depth, and five feet in diameter. In theſe they are ſpread out to the depth of two inches, and when they are cold enough to be worked with the hand, the wheat flour is gradually thrown in and well mixed with the haricots, until the whole of the before-mentioned quantity has been uſed. When the maſs becomes too dry, ſo that the flour does not mix well with the haricots, a little of the hot water of decoction is added.

—and thinly ſpread out in a veſſel covered up.

A peculiar mouldineſs is ſuffered to come on by admitting the air:

The whole being well mixed, the maſs is then ſpread abroad in the veſſels before-mentioned. taking care that its depth ſhall not be more than an inch or an inch and a half; and the maſs is then covered by a lid which fits exactly. When the maſs begins to become mouldy and heat is diſengaged, which happens after two or three days, the cover is then raiſed by putting two ſticks beneath it, in order that the air may have free acceſs. During this time a rancid odour exhales, and if the maſs becomes green, it is a ſign that the whole goes on properly; but if it begins to be black, which muſt be carefully noticed, the lid muſt be elevated ſtill more, in order that the maſs may have more air. If it once becomes black, the whole is ſpoiled.

As ſoon as all the ſurface is covered with green mouldineſs, which uſually happens in eight or ten days, the cover is taken off, and the compound is expoſed to the ſun, and the air for ſeveral days. When it has become as hard as a ſtone, it is cut into ſmall fragments, which are thrown into an earthen veſſel, upon which the 250lbs. of water, having the 50lbs. of ſalt firſt diſſolved in it are poured. The whole is then well ſtirred together, and notice is taken of the height at which the water ſtands. If it be not convenient to put all the mixture into one veſſel, a number may be uſed, taking care that the materials be proportionally diſtributed in each. The veſſel thus filled is placed in the ſun, and its contents ſtirred up regularly every morning and evening, and a cover is put on at night to defend it from the cold, as well as to prevent any rain from finding entrance either by day or night. The hotter the ſun the ſooner will the ſoy be completed. The proceſs is ſeldom undertaken but in the ſummer, notwithſtanding which it laſts two or three months.

—the compound is then dried,

cut in pieces and mixed with ſalt water;

and in a veſſel placed in the ſun and daily agitated

for two or three months.

As the maſs diminiſhes by evaporation, well-water is added; and this digeſtion is continued till the ſalt water has entirely diſſolved the flour and the haricots. The veſſel is ſtill left for ſome days in the ſun, in order to compleat the ſolution ſtill more effectually, as the good quality of the ſoy depends upon this circumſtance; and the daily ſtirring or agitation is continued to the very laſt.

When the fluid has become ſmooth and homogeneous

When at length the maſs has become very ſucculent and oily, the whole, as well the thick as the more fluid portions, is poured into bags, through which the ſoy is preſſed, and is then clear and ready for uſe. It is not afterwards boiled, as Mr. Ekeberg pretends. It is to be kept in bottles well corked. The Chineſe who deal in this article keep it in large pitchers well cloſed. Before it is ſtrained in the preſs the ſoy is of a deep brown colour, but it afterwards becomes black.

it is ſtrained by preſſure and is ſoy.

The Chineſe alſo prepare two kinds of ſoy from the dregs which remain. The firſt time they add 150lbs. of water and

Inferior ſoy from the dregs.

30lbs. of ſalt, and after having preſſed the maſs, they again add 100lbs. of water and 20lbs. of ſalt, always proceeding as before deſcribed.

The two laſt kinds of ſoy are not ſtrong, but very ſalt, more eſpecially the latter, which is alſo lighter coloured. Theſe two kinds are the moſt common in China, and are uſed both by natives and Europeans. The differences of price are as 8.4.1.

This proceſs has been often verified,

In this manner I prepared in 1759, at my reſidence in Canton, the whole of the ſoy I made uſe of, and I alſo brought ſeveral bottles with me to Sweden. It was ſucculent, oily, moderately ſalt, and very different from what is commonly ſold in Europe. With regard to its taſte, it might be put in competition with that of Japan, which is generally conſidered as the beſt.

by the writer himſelf.

This deſcription is more particularly to be depended upon, as I always made the preparation myſelf; and I can venture to aſſert that this proceſs is the only one for obtaining ſoy of the beſt quality.

Soy contains no ſpice or ſugar, and is ſold in China for three half-pence a pound.

Mr. Ekeberg affirms that the ſoy is boiled with the addition of ſugar, ginger and other ſpices; but this is without foundation, and cannot be true, for a lb. Chineſe of ſoy coſts no more than two candareens of Chineſe money *. This was the common price during my whole reſidence at China, and is too low to admit of theſe ingredients in the preparation. It is alſo the fact that ſoy has no indication either of ſugar or ſpice in its taſte; its principal character is ſaline.

VI.

On the Laws of Galvaniſm. In Letters from C. WILKINSON *and* THOMAS HARRISON, *Eſqrs.*

Windermere Lake, Weſtmoreland, Nov. 8.

DEAR SIR,

Some account of Mr. Gough.

I TAKE the opportunity of a few days relaxation in this romantic and pictureſque ſituation, to trouble you with ſome galvanic obſervations, which principally originate from a very pleaſureable converſation I have lately had with my friend Mr. J. Gough, of Kendal. To you, Sir, this gentleman, as a philoſopher and mathematician, is well known. Although deprived of ſight in very early infancy, it is amazing to obſerve the rapidity with which he proceeds in geometrical inveſtigations. Independent of this very ſtriking acquiſition, he is the beſt botaniſt and natural hiſtorian this country poſſeſſes. To theſe branches of knowledge he adds very conſiderable claſſical knowledge; and in his pneumatic enquiries he places no dependance upon any of the gaſes which he does not prepare himſelf. To converſe with ſuch a perſon is to be improved. To ſome lectures I delivered on galvaniſm a few days ago, at Kendal, I was honoured with his attendance; and in conſequence of the law I was attempting to explain, relative to the powers of galvaniſm on metallic bodies, I was favoured, the ſubſequent day, with the following letter from a very intelligent and well-informed profeſſional gentleman at Kendal, Mr. Harriſon, brother-in-law to Mr. Gough; the particulars of which I dare ſay you will find ſufficiently intereſting to merit inſertion in your valuable Journal.

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* The Chineſe pound or tael is, I think, about 20 ounces, and the candareen is a little more than three farthings, that is to ſay, it is one tenth part of the mace which is valued at eight-pence. N.

To C. WILKINSON.

DEAR SIR, *Kendal, Nov, 5,* 1804.

Inveſtigation of the power of different piles in burning wire.

I MENTIONED to you yeſterday my doubts reſpecting the law of galvaniſm, which you have laid down in Nicholſon's Journal for March 1, 1804; where you ſay, that the igniting powers in batteries of the ſame total ſurface, are as the ſquares of the ſurfaces of the elementary plates, ſingly taken in each: This, if I miſtake not, you repeated in your lecture of laſt night; at which time I expreſſed my doubts to Mr. Gough, and we afterwards examined the data given in the paper before-mentioned; on which we made the following calculations, and found that your concluſion was ſtrictly true according to your data, but that the forces of different batteries expoſing unequal ſurfaces, will be in the ratio compounded of the number of plates in each battery, and the ſixth power of the ſides of the elementary plates ſingly taken in each. The data you have given are the following: 100 plates of four in. ſq. ignited

Inveſtigation of the power of different piles in burning wire.

nited half an inch of wire; 400 plates ignited two inches; and 50 plates of eight inch. ſq. ignited 16 inches of the ſame wire.

Now in order to accommodate theſe numbers to algebraic calculation, we will ſuppoſe a compariſon of three batteries, two of which are compoſed of plates of equal diameters, but differing in number; the third ſhall alſo differ from the firſt and ſecond in the magnitude of its plates, and agree with the ſecond in the number of them.

Let n be the number of plates in the firſt, m the number in the ſecond and third batteries; w and y the diameters of the two ſets of plates. Then, if the power of any ſingle plate bear any conſtant ratio to its ſurface, it will alſo have a conſtant ratio to ſome power of its diameter, becauſe the ſurface is as the ſquare of the diameter in ſimilar figures: let p be that power; let l, k, and z expreſs the lengths of wire which are ignited by the 1ſt, 2d, and 3d batteries: then from the firſt datum, $n : m :: l : k$, becauſe the effect is as the number of equal plates, according to experiment. Again, ſeeing that w is the diameter of the plates in the firſt and ſecond batteries, and y the diameter of thoſe compoſing the third, and p expreſſes the power determining the law of force for plates of different diameters, the force of a plate in the firſt and ſecond batteries, will be to the force of a plate in the third, as $w^p : y^p$, and as the number of plates in the ſecond and third is equal, we ſhall have $m w^p : m y^p :: k : z$, becauſe as $\overset{\text{Plate.}}{1} : \overset{\text{Plates.}}{m} :: w^p : k$, and as $\overset{\text{Plate.}}{1} : \overset{\text{Plates.}}{m} :: y^p : z$, then as $w^p : y^p :: k : z$, but as $n : m :: l : k$; therefore $n w^p : m y^p :: lk : kz :: l : z = \frac{l m y^p}{n w^p}$.

To apply this general theorem in order to determine the value of p in all caſes, we will begin with your own experiment made with equal ſurfaces on different numbers.

Since 100 plates, of four inches, ignite half an inch of wire, and 50 plates, of eight inches ſq. ignite 16 inches, we have $n = 100$, $m = 50$, $w = 4$, $y = 8$, $l = \frac{1}{2}$, $z = 16$; therefore $100 \times 4^p : 50 \times 8^p :: \frac{1}{2} : 16$; but ſince $8 = 2 \times 4$, $8^p = 2^p \times 4^p$, therefore $2 : 2^p :: \frac{1}{2} : 16$, and $16 = \frac{2^p}{2^2} = 2^{p-2}$;

but $16 = 2^4$; hence $2^4 = 2^{p-2}$; therefore $p = 6$; hence the force of plates are as the 6th power of their diameters.

When the ſurfaces are equal, the igniting force is as the 4th power of the ſides of the individual plates; for in this caſe we have $n w^2 = m y^2$, and $n : m :: y^2 : w^2$; therefore the theorem $n w^6 : m y^6 :: l : z$, becomes $y^2 w^6 : w^2 y^6 :: l : z$; that is, $w^4 : y^4 :: l : z$, which is the law in your particular caſe.

This theorem will be found uſeful in eſtimating the force of batteries in general; as an example of which we ſhall calculate, by means of it, the force of your intended battery, conſiſting of 50 plates, each 24 inches diameter. In order to determine the quantity of wire that will be ignited by ſuch a battery, we have $n = 100$, $m = 50$, $w = 4$, $y = 24$, or ſix times w, $l = \frac{1}{2}$; therefore $100 \times 4^6 : 50 \times 6^6 w^6 :: \frac{1}{2} : z$; that is, $2 : 6^6 :: \frac{1}{2} : z$, and $z = \frac{6^6}{4} = 36^2 \times 9$ inches, or 972 feet.

I am, Dear Sir,

Your's reſpectfully,

THOMAS HARRISON.

Stramongate, Kendal.

Experiments on the decompoſition of water.

YOU will obſerve, Mr. Editor, from the above theorem, that my ſtatement of the powers of two different galvanic arrangements, equal in their metallic ſurfaces, being as the ſquares of the ſurface of two individual plates, or, which is the ſame, as the 4th power of the diameter of each plate, is correct only where the metallic ſurfaces are equal; when they are not equal, then the powers are as the 6th power of the diameters, or as the cubes of their reſpective ſurfaces.

In a ſhort time you will have the pleaſure of a paper from Mr. Gough, relative to the decompoſition of water by galvaniſm: With this view we tried together the following experiments, the reſults of which tend to favour the explanation I have attempted in the Elements I ſometime ſince publiſhed.

For this purpoſe I employed 270 plates, containing a ſurface of 6720 ſquare inches, and connected with the poſitive and negative ſides by wires of platinum. Theſe platinum wires were inſerted in a diſh of water at eight inches from each other; the poſitive wire diſengaged oxigen gas, the negative wire hidrogen gas. The decompoſition took place very ſlowly; in proportion as the wires were approximated, the decompoſition increaſed in the ſame ratio.

The platinum wires were then inſerted into a capillary tube whoſe bore is about $\frac{1}{20}$ of an inch, filled with water; no ſenſible effect was produced till the wires were placed at the diſtance of half an inch, and then only a ſolitary bubble became diſengaged from the negative wire, never completely detached, but hovering round the extremity, not being ſet at liberty by the evolution of another particle.

When the platinum wires were inſerted in a tube whoſe bore is $\frac{3}{8}$ of an inch, at the diſtance of ſix inches a ſlight decompoſition was obſerved, and which neceſſarily increaſed in proportion as the wires approached each other.

When the platinum wires were inſerted in different glaſſes of water, and the extremities of a frog deprived of its integument conſtituted the connecting medium between the two glaſſes, or a wet piece of cloth, at the diſtance of three inches a ſlight decompoſition was obſerved.

When two wires are connected with the different ends of a galvanic battery, the wires immediately participate, as to their electrical intenſity, with the reſpective plates with which they are in contact.

Inveſtigation of the laws of decompoſition of water by galvaniſm.

Let us ſuppoſe P the wire from the poſitive end of the battery, and N the negative. *Pl. XVI. Fig.* 2. Let us ſuppoſe that the galvanic arrangement is ſuch, that, when the wires are placed at the diſtance *a*, *a*, the decompoſition commences. If the wires be placed at *b* and *b*, the decompoſition is conſiderably increaſed; and this increaſe will go on, till the maximum will be at *d*, *d*, when the wires are infinitely near each other without being in contact.

The poſitive wire, according to its electrical intenſity, produces a correſponding ſtate in all fluids with which it is in contact, proportionate to their capacities; ſo alſo the negative wire; and it is evident from the principle of electrical charges, that the influence of each muſt extend to equal diſtances; it will therefore follow, that when at *a*, *a*, ſuppoſing theſe points the apices of two equal cones, the areas of their reſpective baſes will be the leaſt poſſible, and conſequently, in their concourſe at *i*, will only influence a ſingle particle of water. When the wires are placed at *b*, *b*, then *c i* will expreſs

Inveſtigation of the laws of decompoſition of water by galvaniſm.

preſs the radius of the circular baſe (ſuppoſing $b c = a i$), and conſequently a greater proportion of water ſubjected to the influence; and therefore evidently when at *d*, *d*, or infinitely near, the maximum of decompoſing effects muſt take place. This we obſerve is conſonant to experiments.

If a particle of water conſiſts of a particle of oxigen and of hidrogen in a ſtate of chemical union, and if an agent be employed ſufficiently ſubtle to penetrate the conſtituent particle of water, a ſeparation will take place. Thus caloric will deſtroy the aggregation of the conſtituent particles of many ſubſtances, without producing any decompoſition; in this caſe it only acts by penetrating the interſtices formed by the conſtituent particles, without pervading the ſubſtance of the conſtituent particle itſelf. Thus the portions of water, although participating of the reſpective electrical intenſities of the two wires, as yet undergo no decompoſition; as yet it has not completely pervaded the particle of water. When a particle of water is influenced on one ſide by the poſitive wire, and on its other ſide by the negative, then the particle of water becomes a ſmall Leyden phial; and if the energy of the apparatus ſhould be adequate to overcome the reſiſtance between the two ſides of the particle of water, a ſeparation is produced, and the water becomes reſolved into oxigen and hidrogen.

This deſtruction of the water will take place at *i*, the intermediate diſtance between *a*, *a*; becauſe, when thus remotely ſituated, only a ſingle particle of water becomes thus operated upon; when at *b*, *b*, a greater number of particles of water become thus influenced, and conſequently the decompoſition is more rapid.

If water placed in a capillary tube be employed, it is evident, however near the platinum wires are approximated, the areas of the baſes of the reſpective cones will be not at all increaſed, and, conſequently, the maximum of its power will be confined comparatively to a ſolitary particle of water. The inſtant a particle of water becomes thus ſeparated into its elementary portions, it might be expected that theſe portions would be evolved at *i*, as being the point of concourſe between the poſitive and negative ſtates; but in this form they are not in the gaſeous ſtate; they require caloric, &c.; and that they do not very readily acquire this other principle neceſſary to conſtitute them gaſes, is evident from the oxigen, if an oxidable metal be employed at the poſitive end, entering into combination with the metal, inſtead of being diſengaged in an aeriform ſtate.

Water is a conductor of electricity; its capacity for this principle being in proportion to its conducting powers. When converted into vapour, its conducting powers are increaſed, and ſo alſo in the ſame proportion its capacity, as is evident from water, when undergoing this change, abſtracting electricity from ſurrounding bodies.

When a particle of water is reſolved into its elementary portions, a change takes place in their capacities for electricity, caloric, &c. at the very naſcent inſtant, Oxigen we know, from other experiments, has its capacity for electricity increaſed, and muſt acquire this principle before it can aſſume a gaſeous form. This acquiſition will be effected with the greateſt readineſs in the direction towards the poſitive wire; while the hydrogen particle having its capacity diminiſhed, will be ſimilarly determined towards the negative wire.

For a future Journal I will trouble you with ſome experiments relative to this change of capacity. I am anxious firſt to peruſe Mr. Gough's intended paper, from whoſe philoſophical abilities much may be expected.

Having extended this paper to a greater length than I at firſt expected, I ſhall defer, till another opportunity, a reply to Mr. Cuthbertſon's obſervations, and flatter myſelf I ſhall be enabled to clearly point out the error into which Mr. C. has fallen.

Cure of rheumatiſm and palſy by galvaniſm.

Although your Journal may not be deemed the proper medium for any medical communications, as I have lately witneſſed the very powerful effects of galvaniſm in rheumatic and paralytic affections. Whatever may prove of benefit to mankind will, I am perſuaded, be readily admitted into your valuable Work.

I am, Dear Sir,
Your's, with the greateſt reſpect,
C. WILKINSON,
Surgeon, 19, Soho Square, London.

Remark

VII.

Remark upon an Aſſertion of LAVOISIER, *which has been repeated by eminent Chemiſts. By* P. PREVOST, *Profeſſor at Geneva, and Correſpondent with the National Inſtitute of France.* *

"IT is a conſtant phenomenon in nature . . . . . that when any body whatever is heated . . . . . its dimenſions become increaſed in every direction . . . . . If after having heated a ſolid body to a certain point, and by that means ſeparated its particles more and more, it be then ſuffered to cool, theſe particles will approach each other, according to the ſame gradation by which they were before ſeparated; the body will paſs through the ſame dimenſions it before poſſeſſed; and if it be reduced to the ſame temperature as at the beginning of the experiment, it will reſume its original volume. But as we are very far from being able to produce an abſolute ſtate of cold; but on the contrary, as there is no degree of refrigeration which we cannot ſuppoſe to be capable of being further augmented, it muſt follow that we have not yet ſucceeded in bringing the particles of any body as cloſely together as is poſſible, and conſequently that *the particles of no body in nature can be in a ſtate of contact*; a very ſingular concluſion, but to which nevertheleſs it is impoſſible to refuſe our aſſent."—*Elementary Treatiſe of Chemiſtry, by Lavoiſier, Vol. I. at the commencement.*

Aſſertion of Lavoiſier, that ſince bodies are contractible by cold, their particles are not in contact.

The ſame aſſertion is repeated ſeveral pages further, where an example is taken of a veſſel filled with ſmall leaden ſhot. "The balls touch each other," ſays our author, "whereas *the particles of bodies do not touch*, but are always kept at a ſmall diſtance from each other by the effort of caloric."

The ſame aſſertion repeated.

The authority which this celebrated chemiſt enjoys in the learned world, is probably the only cauſe why this concluſion has been adopted. It is at leaſt certain that it is not legitimate, though it has been ſo often repeated; and more particularly in a late work, no leſs remarkable for the depth of reaſoning and extent of knowledge which it diſplays. Berthollet, by Berthollet,

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* Annales de Chimie, Vol. L. p. 58.

thollet, in his *Eſſai de Static Chimique*, Tom. I. p. 24, parag. 2,* has theſe words: "Coheſion is the effect of that affinity which the particles exerciſe on each other, and keeps them at a diſtance, determined by the equilibrium of this force with thoſe which are oppoſed to it; for the property poſſeſſed by the moſt compact bodies of undergoing a diminution of volume by reducing their temperature, proves that there is no immediate contact between their parts."

Apology for Berthollet,

After all, it is not ſurpriſing that an author, who has undertaken and ſucceſsfully executed a work of ſuch immenſity with regard to chemiſtry, ſhould refer to one of his moſt celebrated predeceſſors for the ſolidity of an argument which relates to the principles of natural philoſphy; and this conduct is ſo much the more natural, as the argument never having been conteſted, though frequently quoted, a conſiderable preſumption ariſes in its favour, to diſpenſe with any careful examination in caſes where it is not intended to be farther applied. Now it does not appear that any farther uſe is made of the propoſition throughout the great work to which our attention is now directed. I therefore conſider the paſſage here extracted, rather as the occaſional mention of a ſingular paradox, than as a theſis which the author was deſirous of eſtabliſhing.

whoſe authority may nevertheleſs aſſiſt in eſtabliſhing this error.

Let this be as it may, it will notwithſtanding follow, that the mere incidental mention of an aſſertion by an author ſo juſtly reſpected, will be conſidered as a proof of its truth; and that the numerous diſciples of this great maſter will repeat it with confidence. It is therefore deſirable that its want of foundation ſhould be inſiſted on, and that it ſhould be directly refuted in a Journal of extenſive circulation; more particularly amongſt thoſe who are moſt likely to be miſled by ſuch an error.

The author does not diſcuſs the propoſition itſelf, but only inſiſts that expanſion and contraction prove nothing.

Before we proceed, it will be proper to make an eſſential diſtinction. The aſſertion is as follows: *The particles of bodies do not touch each other*; or in other terms, *there is no immediate contact between the elementary parts of bodies.* Now, to ſpeak with propriety; I do not at all intend to deny this aſſertion, neither do I propoſe to eſtabliſh it. All that I intend to do is, to ſhew that it is not legitimately concluded from the principle whence it has been deduced; namely, the diminution of volume which accompanies depreſſions of temperature. Now I ſay that this diminution does not at all prove that the elements of bodies have no immediate contact, and conſequently that this laſt aſſertion is ſo far gratuitous. And this is the whole of what I deſign to prove.

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* See Lambert's Tranſlation, Vol. I. p. 2.

We can eaſily conceive expanſion without ceſſation of contact.

If it be true that there are numerous conceptions and examples which are evident and eaſy to be given, of bodies ſubject to dilation and condenſation without their parts ceaſing to be in immediate contact, it muſt neceſſarily be concluded, that dilation and condenſation can afford no proof in this reſpect. For why ſhould we refuſe to admit ſome of theſe conceptions, or to apply ſome of theſe examples to the caſe of the elements?

1. Inſtance: the legs of compaſſes may open and ſhut.

1. Conceive the particles to be elongated and united by their extremities, like the legs of a pair of compaſſes; and they may turn with regard to this point of union, as a centre, and produce condenſations and dilations of the whole apparent maſs of the body ſucceſſively.

2. Sponge, &c. may be compreſſed or enlarged.

2. A dry ſponge, or fruit, or mucoſity, being plunged in water, will dilate without any ceſſation of the ſenſible contact; and on the contrary, the ſame bodies, when wetted, if expoſed to become dry, will undergo condenſation.

3. Expanſion in cryſtals.

3. The example of the dilatation of ice and ſome metals by cryſtallization, an example formally remarked, ſtudied, and well explained, particularly by the latter of the two learned chemiſts I have cited, muſt ſhew the falſity of the aſſertion which I refute.

Upon reading the work of Berthollet, and taking notes of ſuch things as I was deſirous of retaining, or concerning which I ſaw reaſon to doubt, I wrote what is here tranſcribed, *i. e.* the ſummary indication of the three conceptions, or examples, proper to elucidate this ſubject.

Similar remarks by Leſage.

I had no intention of ſubmitting my remark to its natural judges, when five or ſix weeks afterwards, being employed in a very different occupation, I found in the papers of a learned * philoſopher, which were entruſted to me at his death, a card which contained the ſame remark, but in a more abridged form, and by ſimple indication. As it offers a variety of examples and conceptions of the ſame nature as thoſe which I have given, though nevertheleſs different, I ſhall here give them verbatim:

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* George Louis Leſage, Correſpondent of the Academy of Sciences of Paris, afterwards Correſpondent of the National Inſtitute, Member

Note of G. L. Leſage.

"Methods of ſhewing that this conſequence does not follow:

Expanſion and contraction, 1. Of the hands; "1. Four fingers of one hand introduced more or leſs between the four intervals of the fingers of the other hand.

2. Combs, &c. "2. The ſame with a couple of bruſhes, or cards, or combs.

3. Cotton, &c. "3. Carded cotton, or hair, and ſoap lather.

4. Snow. "4. Snow.

5. Oſcillation. "5. Agitation or oſcillation, which renders the ſame particles alternately contiguous and ſeparate more than a thouſand times per minute."

Concluſion. The peruſal of this note determined me to publiſh mine; not only becauſe I found myſelf authorized by this coincidence, but alſo to render a firſt homage to the memory of a philoſopher no leſs modeſt than ingenious, and to begin in ſome reſpect to execute his will, by publiſhing at leaſt one of his notes without making any change. The laſt conception offered in this laſt ſhort note, will probably induce ſome philoſophers to reflect again upon this ſubject, and may perhaps lead them towards the opinions of the writer.

Member of the Royal Society of London, and of ſeveral other learned Societies, died at Geneva, the 28th Brumaire, in the year XII. (Nov. 19, 1803). His principal writings have not yet been publiſhed. I ſhall ſoon give a ſhort notice, as well as ſome details of his literary life, proper to facilitate the peruſal of his works, and eſtabliſh their originality. P.

VIII.

Account of a Memoir on chamoying of Leather.
By M. SEGUIN. *

M. SEGUIN, who has already publiſhed ſeveral intereſting works on the arts, relating to the preparation of ſkins, has lately read to the Inſtitute a firſt memoir on chamoying, from which we ſhall give an extract.

The author ſtates, that the art of chamoying conſiſts in diſpoſing the ſkins to receive the oil; in impregnating them with it by different operations, of which he probably reſerves the details for a ſecond memoir; in then cauſing them to undergo a ſpecies of fermentation; in expoſing them to the air; and, laſtly, in taking from them, by means of potaſh, the exceſs of oil which is uſeleſs to them.

Mechanical operations of chamoying.

He afterwards paſſes to the chemical examination of a chamoyed ſkin.

He found that this ſkin did not undergo any alteration by a long boiling in water; but that, if any acid whatever is added (M. Seguin made uſe of ſulphuric acid), the ſkin diſappeared entirely; that a certain quantity of a concrete oil ſwam on the ſurface of the liquid; that the liquor contained gelatine; and that, by its evaporation, it depoſited cryſtals of ſulphate of potaſh. He alſo aſcertained, that on pouring gelatine into a ſolution of ſoap, an inſoluble precipitate is obtained, which, treated with an acid, comports itſelf exactly like a chamoyed ſkin.

Chemical examination of chamoyed leather.

Gelatine poured into a ſolution of ſoap, forms a precipitate which reſembles a chamoyed ſkin in its chemical properties.

Theſe reſults, and the conſiderations to which the expoſition of the principal operations of chamoying have given riſe, induced M. Seguin to conclude that the ſkins, in the fermentation which they undergo, yield a part of their oxigen to the oil; that the potaſh employed to cleanſe them, forms a ſoap with the oxigenated oil; that one part of this ſoap, by combining with the diſoxigenated ſkin, produces the inſoluble ſubſtance which makes the ſkin chamoyed; and that the other part ſerves to conſtitute that greaſe which remains in the leather.

Theory of the operation.

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* From Bulletin des Sciences, Tom. III. p. 209.

This work is the more intereſting, becauſe hitherto no one has conſidered chamoying in a chemical view, and becauſe the analyſis to which M. Seguin has ſubmitted his reſults, renders it ſuſceptible of acquiring that perfection which the diſcoveries of this chemiſt have already given to the art of the tanner.

IX.

Analyſis and Decompoſition of a Liquor employed to render Stuffs impermeable to Water. By M. Vauquelin.[*]

It is known that, for ſome years, different perſons have been ſucceſsfully employed in rendering ſtuffs impermeable to water, an object of great importance in the clothing of ſoldiers and ſailors.

The inventors of this proceſs have hitherto kept the means they make uſe of ſecret: there was only reaſon to ſuppoſe that ſome fat oil was the baſis of their receipts, but experiment has not yet developed it.

A bottle of this preparation, of which the efficacy was known, having accidentally come into my hands, gave me a deſire to inveſtigate its compoſition; but before ſtating the plan which I followed with this view, I ſhall deſcribe its phyſical properties.

Phyſical characters of a liquor for rendering ſtuffs impermeable to water.

It is a white liquor, milky and opake, of a bitter taſte, and ſmelling like ſoap; on its ſurface there is a ſpecies of cream reſembling that of milk, and it ſtrongly reddens the tincture of turnſole. From theſe properties I thought it was ſimply a ſolution of ſoap, of which it ſtill retained the ſmell and taſte, which had been decompoſed by an acid; but ſubſequent experiments ſoon convinced me that there was ſomething more in it.

Chemical examination.

A turbid part is ſeparated by repeated filtration.

Firſt Experiment.—To find whether I could ſeparate the white matter which rendered the liquid turbid, by filtration, I put a certain quantity of it on bibulous paper: it paſſed turbid and milky for a long time, but, by returning it ſeveral times on the ſame filter, I ſucceeded in obtaining it as clear as water; and I afterwards examined the liquor, and the matter which remained on the filter, ſeparately.

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[*] From Bulletin des Sciences, Tom. III. p. 210.

Second Experiment.—If my conjecture was well founded, I ſhould only find in this liquor the baſe of a ſoap united to an acid, of which there was a ſuperabundance. My firſt care was to ſatisfy myſelf of the nature of this acid, and that which its taſte had already indicated to me, was confirmed by muriate of barytes producing in it an abundant precipitate, inſoluble in nitric acid: thus I was convinced that the liquor contained ſulphuric acid; but, on the other hand, ammonia having produced in this liquor a white, flocculent, ſemi-tranſparent precipitate, I ſaw that it contained ſomething more than the ſalt reſulting from the decompoſition of ſoap.

Contains ſulphuric acid.

Ammonia forms a white precipitate in it.

Third Experiment.—I then precipitated a certain quantity of this liquor; I waſhed the precipitate and dried it: as it had all the phyſical characters of alumine, I combined it with ſulphuric acid; I added to it a little ſulphate of potaſh, and, by a ſlow evaporation, obtained ſome very fine alum. We have therefore already found in this liquor alumine and ſulphuric acid, doubtleſs combined in the ſtate of alum.

Contains alumine, probably in the ſtate of alum;

Fourth Experiment.—It was now requiſite to know whether the liquor from which I had ſeparated the alumine, did not ſtill contain ſome other ſubſtance, and I therefore ſubmitted it to ſome trials by the re-agents, among which the oxigenated muriatic acid and the infuſion of nut-galls enabled me to diſcover a new body: the firſt rendered the liquor turbid, and ſoon afterwards produced white flakes in it; the ſecond produced yellowiſh-white flakes, in much greater abundance than thoſe ariſing from the effect of the muriatic acid; whence I ſuſpected that, beſides the matters mentioned above, this liquor contained an animal matter, and more particularly gelatine.

and alſo an animal matter,

Fifth Experiment.—To be better ſatisfied of the nature of this ſubſtance, I evaporated the liquor to dryneſs, by means of a gentle heat; I obtained a yellowiſh ſalt, of a bitter taſte, which, by being re-diſſolved in water, left a voluminous yellow matter, in the form of flakes, very glutinous, and acquiring, by drying, a ſort of elaſticity. This ſubſtance, placed on burning coals, ſwells and exhales white fumes, which have the odour of ammonia and fetid oil, ſuch as are generally given by animal matters.

I no

which is gelatine.

I no longer doubted that a certain quantity of animal gelatine had been put into this compoſition, with the intention of giving greater viſcoſity to the liquor, and enabling it to ſupport, for a longer time and more effectively, the parts of the oil in ſuſpenſion. It is probably by heat and by a commencement of decompoſition, that the animal gelatine had become inſoluble in water; but I perceived that the liquor containing the ſalt alſo held ſome of it in ſolution, for the muriatic acid and infuſion of galls ſtill formed precipitates in it, only leſs abundant than the firſt time.

Examination of the fat ſubſtance;

Sixth Experiment.—In this experiment I endeavoured to learn the nature of the fat ſubſtance retained in the filter, and of which I have ſpoken above: my intention was principally to diſcover whether it held ſome other ſubſtance in combination.

For this purpoſe I burned it with the filter in a platina crucible; it exhaled a vapour ſimilar to that of tallow or the oils; it left ſome aſhes, of which the filter had furniſhed a part: I found a ſmall quantity of alumine in them, which could only proceed from the oil, for the bibulous paper did not contain an atom. I am alſo of opinion, that, in addition to the alumine, this oil contained a ſmall quantity of animal matter, but I cannot poſitively aſſert it.

which retained ſome alumine in combination.

Thus, notwithſtanding the exceſs of acid which exiſted in the liquor, the oil in precipitating carried with it, and retained in combination, ſome alumine, and probably ſome animal gelatine.

It is a combination of oil, alumine, and animal gelatine;

Hence the ſubſtance which, by uniting with ſtuffs, renders them *impermeable* to water, is not the oil alone, but a combination of this ſubſtance with the alumine, and probably with the animal gelatine, which renders this property more durable.

and yields a little ſoda and ſulphate of potaſh.

Seventh Experiment.—The liqnor which I had ſucceſſively deprived of the oil, the alumine, and, in part, of the animal matter, by the different proceſſes mentioned above, gave me, by a ſlow evaporation, cryſtals of a ſalt compoſed of ſoda and ſulphate of potaſh.

Another analyſis.

Eighth Experiment.—I made a better analyſis of this liquor by another proceſs, which I ſhall detail very briefly.

I precipitated the alumine and the oil by lime-water; I collected, waſhed, and calcined the precipitate: that which remained in the crucible was alumine and lime.

The liquor, from which theſe matters had been ſeparated, evaporated to a certain degree, yielded ſulphate of lime, a certain quantity of animal matter, become inſoluble by the deſiccation of the liquor, and, finally, ſulphate of ſoda and potaſh, containing alſo animal gelatine, ſoluble in water.

Manner of preparing the liquor.

The following is the manner in which I conceive this liquor was prepared, *with the exception of the proportions:* Soap and glue, or any other gelatine, was diſſolved in water; a ſolution of alum was mixed with the ſolution of theſe ſubſtances, which, by its decompoſition, formed in the mixture a flocculent precipitate, compoſed of oil, alumine, and animal matter; afterwards ſome dilute ſulphuric acid was added, to rediſſolve the alumine in part, to render the precipitate lighter, and to prevent it from falling: but the alumine being once combined with the oil and animal matter, will not diſſolve again entirely in the ſulphuric acid, which is the reaſon why the oil continues to be very opake, and neither riſes nor precipitates: it will be underſtood, that too much ſulphuric acid is not to be added. I do not know if this is preciſely the manner in which it is made; I only know, that by following this proceſs, I ſucceeded in compoſing a liquor exactly reſembling it, and which poſſeſſed the ſame properties.

X.

New Experiments on Abſorption by Charcoal, made by Means of a new Machine. By C. L. Morozzo.*

First notice of the invention.

1. In the ſecond memoir which I publiſhed on charcoal, in 1783,† I mentioned my intention of employing its property of abſorbing the gaſes, to form a good eudiometer. I immediately ſet about the conſtruction of an inſtrument with which I made ſome experiments; but the agitation of the times obliged me to abandon my purſuits: a philoſopher, however, to whom I had ſhown this inſtrument, took the liberty of publiſhing it, notwithſtanding its imperfections, and without naming the

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* From the Journal de Phyſique, Floreal, An. XII.

† Journal de Phyſique, p. 376.

author *. Let us paſs over theſe unpleaſant events, as well as the alterations and corrections which I made to bring the inſtrument to its preſent ſtate: the deſcription which follows, and the figure will be ſufficient to ſhow what it now is.

Deſcription of the Inſtrument. (*Plate* XIV.)

Deſcription of the inſtrument.

II. A B is a tube of clear glaſs, three lines and a half in diameter and eighteen inches high: to this tube a ſtop-cock C is fixed, to ſhut the communication, by means of the key D, ſo that the tube may be filled with water or mercury, and the gas to be examined may be afterwards paſſed in. To this ſtop-cock C a large one E is ſoldered, an inch and a half in diameter, and almoſt three inches long. The handle F F ſerves to turn the key of the ſtop-cock E; this key has a hole, one inch and three lines in depth, and ſeven lines in diameter, (*Fig.* 2.) in which the charcoal is placed. This key is perfectly tight, and, when turned, communicates with the cock C, *Fig.* 3, not permitting the paſſage of any air whatever. The large ſtop-cock is encloſed in a wooden frame, and well ſecured by means of the nut H, which confines the lower end of the ſtop-cock E behind the board; the lower extremity of the tube dips into a veſſel of mercury I I; the board M M keeps the inſtrument perpendicular; N N are pads which ſerve to raiſe the cup filled with mercury; O O is a ſcale mounted on a ſlip of wood, which is graduated to inches and lines. The endleſs ſcrew P ſerves to raiſe or lower this ſcale, to bring it to the level of the mercury in the cup.

III. After having uſed the inſtrument for ſome time, it is neceſſary to unſcrew the keys to clean them, for, by uſe, duſt, charcoal, or aſhes will get in, which deranges the inſtrument, and ſuffers air to paſs.

IV. From ſeveral experiments, I found that the ſtop-cock D C, which is attached to the cryſtal tube ſhould be of ſteel, for on examining the reſidual air, or filling the tube with mercury to examine any gas, the mercury will at length attack the copper.

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* The author does not name the perſon here alluded to; and in a note he gives a deſcription of this firſt imperfect inſtrument, referring to the figure ſo publiſhed. This is omitted, becauſe of little utility and not intelligible without that figure. N.

Propoſed improvement.

V. I alſo think it right to propoſe a ſmall alteration: it is that the extremity of the tube which is plunged into the bowl of mercury, ſhould be a little more bent, that the reſidual gas may be transferred with greater eaſe: this ſlight variation, however, is not at all injurious to the inſtrument, becauſe the heights are meaſured by that of the mercury, the level of which is indicated by the ſcale prepared for that purpoſe; its greateſt inconvenience will be to make a larger and more cylindrical bowl neceſſary.

Accuracy of the inſtrument.

VI. This machine was made by J. B. Piana, a very ſkilful mechanic; he conſtructed it with ſuch accuracy, that it preſerved the moſt perfect vacuum for eight days, which is what I am unacquainted with in pneumatic machines.

Manner of uſing the Inſtrument.

Method of uſing it.

VII. Suppoſe I am deſirous of examining the abſorption effected in atmoſpheric air by charcoal. The tube A B is filled with the atmoſperic air of the place in which the machine is: the ſtop-cock D is open, the upper one is cloſed: I place the cup of mercury under the tube, into which I inſert a ſmall glaſs ſyphon, to cauſe the air to take the level of the mercury. If I make uſe of a gas, I pump out the internal gas of the tube with a glaſs ſyringe, to bring the level within the tube to that of the exterior mercury in the bowl. Afterwards, by means of the ſcrew P, I move the ſcale till its zero is alſo at the level of the mercury.

I make a piece of charcoal, weighing, half a dram or thirty-ſix grains, red-hot, and with tongs place it in the cavity G of the large ſtop-cock; I then turn the handle F F, and in a few minutes the mercury will be ſeen to aſcend more or leſs in the tube, according as the air is more or leſs pure.

If it is wiſhed to examine other gaſes, or atmoſpheric air brought from another place, then cloſe the key D, and fill the tube A B with water; afterwards diſplace the water with the gas, in the common way by means of a transferrer; bring it to the mercury, and, by means of bibulous paper, abſorb the water perfectly; then wipe the outſide of the tube carefully with warm flannel, and leave the inſtrument at reſt for two hours: or otherwiſe make uſe of a mercurial apparatus, which is preferable.

Inconveniences of the Inſtrument.

Inconveniences.

VIII. Nothing is more certain than the property poſſeſſed by charcoal of abſorbing a more or leſs conſiderable quantity of gas, and of atmoſpheric air; the experiments which I have publiſhed are a complete demonſtration of it; but I then made uſe of white glaſs tubes, hermetically ſealed at the upper extremity, and paſſed the charcoal under the mercury, by which its interſtices were filled with that fluid. At preſent, on endeavouring to make comparative experiments with my new machine, which I was deſirous ſhould be a perfect eudiometer, I found that the cavity G, in which I placed the charcoal, and the ſmall ſpace between the two keys of the ſtop-cock, contained air; that by the heat of the charcoal this air was dilated, and partly expelled, and that conſequently, on opening the key of the ſtop-cock, a ſmall abſorption was occaſioned by the vacuum thus made, and the mercury then roſe. To convince myſelf, and to diſcover at what quantity this abſorption might be calculated, the apparatus being arranged as for other experiments, inſtead of charcoal I introduced a piece of red-hot pumice-ſtone (I gave the preference to this ſubſtance, becauſe I was certain that it had not the property of abſorbing the air, and alſo that it would not take a greater heat than charcoal): on turning the key an abſorption of an inch took place, and from ſeveral experiments, I am convinced that this quantity is uniform within half a line.

Correction of the Inſtrument.

Correction of the inſtrument.

IX. From the experiment made with the pumice-ſtone, it is obvious that if an inch is deducted from the total abſorption, the real abſorption produced by the charcoal will be obtained.

Beſides, in comparative experiments this imperfection cauſes no error; becauſe as they are all conducted in equal circumſtances, the variations produced, either by the different gaſes, or by the different quality of the charcoal made uſe of, will be always proportional.

X. With this inſtrument I made ſeveral experiments which I ſhall now relate.

Order of the experiments.

XI. I began by examining the action of charcoal when cold, and afterwards at a low heat; I then examined the abſorption by charcoal expoſed, for a greater or leſs length of time, to the light of the ſun; this enabled me to aſcertain the quantity of light and caloric which enter into the charcoal.

XII. The experiments on atmoſpheric air, which is more or leſs abſorbed according to its purity, follow next.

XIII. After theſe are the experiments on different gaſes.

XIV. Oxigen gas gave me very ſingular reſults, diametrically oppoſite to thoſe I had obtained in tubes hermetically ſealed, and when I paſſed the charcoal through the mercury, which deranged me greatly in the conſequences I deduced from it.

XV. After this article will follow ſome conſiderations on the quality of the charcoals made uſe of, which prove that their property of abſorbing is different according to their different qualities.

XVI. I afterwards ſhow that my inſtrument may be uſed in many caſes, if inſtead of mercury, coloured water is employed, but with requiſite modifications.

XVII. The charcoals which had been employed in the experiments all acquired a greater weight: I endeavoured to extract the air which they had abſorbed.

XVIII. I alſo mention the trials which may be made by ſaturating the charcoal with different ſubſtances, having only made four experiments.

XIX. I then proceed to the concluſion, in which I endeavour to give the explanation of theſe experiments. The opinion which I had adopted in the ſecond memoir, receives additional confirmation from it; but the reſults of the experiments I made on oxigen gas having changed the ideas I had formed, I am not willing to hazard an explanation at preſent. Philoſophers will therefore be contented with having new facts and new experiments.

The experiments which I detail were repeated ſeveral times, and their accuracy may be depended on, as I can aſſure my readers, that I have always conſidered truth as the brighteſt ornament of a philoſopher.

Experiments made with my new Inſtrument.

Experiments with the new inſtrument.

1ſt. The charcoal which I generally employed was that of beech-wood; the weight of the pieces half a dram, that is to ſay, thirty-ſix grains.

2d. The tube of my glaſs was eighteen inches long, from the bottom of the key to the ſurface of the mercury, the level of which is indicated by the moveable ſcale. The diameter of the tube is three lines and a half: it contains one ounce and ſeven drams — five grains of water, or a volume of atmoſpheric air of four grains and a half at a temperature of +10°.

3d. Being deſirous of throwing a light upon the property poſſeſſed by charcoal of abſorbing a part of the atmoſpheric air, as well as of ſeveral gaſes, by means of my new inſtrument, I made a great number of experiments, which I ſhall now deſcribe.

4th. Charcoal, when cold, has the property of abſorbing ſome ſmall portions of atmoſpheric air, and this abſorption, although very ſlow, is not complete in leſs than twenty-four hours.

On charcoal heated without the contact of fire,

5th. Wiſhing to try whether charcoal to which heat had been communicated without direct expoſure to fire, would ſhow any abſorbent power, I placed a piece in a ſmall matraſs, which I immerſed in boiling water for an hour; having afterwards put the charcoal into the machine, I obtained an abſorption of about three inches.

6th. I boiled oil, in which I left the matraſs for an hour; an experiment with this charcoal gave an abſorption of three inches two lines.

7th A ſmall matraſs containing a piece of charcoal was immerſed for an hour in boiling alkaline lixivium: it gave an abſorption of three inches three lines nearly.

Theſe three experiments ſhow that heat communicated to charcoal, even without the contact of fire, gives it the property of abſorbing a larger portion of air.

and expoſed to the ſolar light.

8th. The preceding experiments induced me to examine whether the light of the ſun would communicate the ſame property to charcoal, I therefore expoſed different pieces to this light, in a white porcelain bowl, and the abſorptions were proportionate to the times which the pieces were expoſed to the light of the ſun *.

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* At the inſtant of putting a piece of charcoal into the machine, I placed another which had been expoſed with the reſt to the ſun for three hours, on the bulb of a thermometer; it only raiſed one degree and a half of Reaumur's ſcale: the heat therefore was not very conſiderable.

After two hours the charcoal abſorbed one inch two lines.

After three hours, one inch eight lines.

After five hours, two inches.

After ſeven hours, three inches nearly.

The light uni with the charcoal.

It is difficult to obtain very preciſe reſults, becauſe the heat of the ſun is greatly varied, according to the wind; but in this caſe it is light as light which unites with the charcoal, for in very cold days when the thermometer in the ſhade was at —6°, I ſtill obtained an abſorption *.

9th. Being deſirous of aſcertaining whether this abſorbent property belonged to charcoal excluſively, I tried ſeveral other bodies, which there was reaſon to believe contained a great deal of the matter of fire.

Experiments loaf ſugar,

10th. I took a ſmall cylinder of loaf ſugar, cold; it did not produce the ſmalleſt apſorption.

A ſimilar piece, which I had heated in a matraſs with boiling water, gave an abſorption of between two and three lines.

A piece of ſugar which had been expoſed to the ſun for two hours alſo gave an abſorption of between two and three lines.

on ſulphur,

11th. I then proceeded to the examination of ſulphur and ſealing wax.

A cylinder of ſulphur of the ſame weight gave no abſorption when cold.

A ſimilar cylinder, which had been expoſed four hours to the light of the ſun, gave an abſorption of about three lines.

and on ſealing wax.

A piece of cold ſealing wax did not give the leaſt abſorption, and a ſimilar piece which had been two hours in the ſun, gave an abſorption of about two lines.

12th. All theſe ſmall abſorptions which were obſerved as well in the ſugar as in the ſulphur and ſealing wax, were only owing to the ſlight heat of theſe bodies, which produces a dilatation in the air of the cavity, and a diſplacing of that portion of the air which is filled by the ſolid body; for a piece of pumice ſtone, of cork, or of any other body, produces the

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* It would be intereſting to examine the abſorptions which might be produced by different pieces of charcoal which had been in the light of the ſun tranſmitted through coloured glaſſes, or by making the different coloured rays fall on the charcoal by means of a priſm, and to obſerve whether the piece expoſed to the red ray would abſorb more or leſs than that which had received the violet ray, and ſo of the others.

ſame

Charcoal alone poſſeſſed of this abſorbent property.

ſame effect. It is therefore in charcoal alone that this abſorbent property reſides, and it is developed to the higheſt degree by heat and light.

Experiments on Atmoſpheric Air.

Experiments on atmoſpheric air.

13th. I then proceeded to examine the effects of incandeſcent charcoal on atmoſpheric air, in my inſtrument, and I repeated the experiments ſeveral times: the air of my garden was conſtantly leſs abſorbed than that of my bed-room, an hour after the windows had been opened, and this was leſs abſorbed than that of my bed-room in the morning before opening the windows: the abſorptions were,

The air of the garden, ſeven inches ſix lines.

The air of the room after the window had been open, eight inches one line.

The air of the room, in which it had not been renovated, eight inches ſix lines.

Former experiments.

14th. In the experiments which I made in the year 1783, with tubes twelve inches in height, and one inch in diameter, I paſſed the charcoal through the mercury; the abſorption of the atmoſpheric air was then always about three inches and ſix lines, that is to ſay, a little more than one fourth: in this caſe it is uniformly greater and about one third. Probably the mercury filled many of the interſtices of the charcoal when it was paſſed through it, which might prevent this greater abſorption.

Air of a privy.

15th. I afterwards examined the air of a privy, which was taken into the ſyringe ſeven toiſes above the ſewer.

The abſorption was eight inches, that is to ſay, nearly equal to that of my room. This did not ſurprize me, becauſe in thoſe compoſitions which contain azote, this gas is not much abſorbed, as we ſhall preſently ſee: beſides, Mr. White with an eudiometer of nitrous gas alſo found that the air of a privy gave him an abſoption equal to that of common air *.

Experiments on the Gaſes.

Experiments on the gaſes.

16th. After having examined the atmoſpheric air, I proceeded to the examination of carbonic acid gas: I filled the

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* See Journal de Phyſique, Tom XVIII. p. 145, and the reflections of Guyton Morveau in his excellent treatiſe on the means of diſinfecting the air.

tube

Carbonic acid gas.

tube with this gas, which I had extracted from powdered marble by vitriolic acid. I placed the incandeſcent charcoal in the machine which effected an abſorption of ſixteen inches ſix lines, that is to ſay, of eleven twelfths of the capacity. This experiment correſponds exactly with that which I made with the tube of twelve inches in height, and one inch in diameter, and in which the charcoal was paſſed through the mercury: the abſorption in that caſe was eleven inches.

17th. I afterwards examined the effect produced on the ſame gas by a piece of charcoal which had remained five hours in the ſolar light. It cauſed an abſorption of ten inches and three lines, which ſurprized me greatly.

18th. But to aſcertain whether charcoal has any peculiar affinity with carbonic acid gas, I tried this gas by placing a piece of cold charcoal in the machine, and the abſorption was nine inches ſix lines.

A mixture of carbonic acid gas and atmoſpheric air.

19th. To diſcover the effect of carbonic acid gas when mixed with atmoſpheric air, I filled the tube, which is eighteen inches high, with nine inches of carbonic acid gas, and nine inches of the air of my room with the windows open: the charcoal effected an abſorption of twelve inches and three lines.

On analyſing this experiment, it agrees perfectly with thoſe related above, for

The abſorption of the air of my room was	8 inches	1 line.
———————— of the carbonic acid gas	16	6
Total	24	7

The half of this is twelve inches three lines and a half, within half a line of the abſorption obtained.

Hidrogen gas.

20th. I afterwards tried the action of charcoal in hidrogen gas, obtained from iron by ſulphuric acid, and the abſoption was three inches and one line: it was accompliſhed in five or ſix minutes, and ſtopped at that height: the effect obſerved in my machine did not ſurprize me, for I had noticed in my firſt memoir, that of all the aeriform fluids, hidrogen gas was abſorbed the leaſt. There is a perfect correſpondence between this experiment, and that which I made in the year 1783, in cryſtal tubes of one inch in diameter and twelve in height: the abſorption was then two inches and one line, that is to ſay, one ſixth of the height; in my new machine, the tube of which is eighteen inches long, the abſorption was three inches and one line, which is alſo the ſixth of the capacity.

21ſt. On trying the ſame hidrogen gas with a piece of charcoal which had been kept about four hours in the bright ſolar light, I had only an abſorption of ſix lines.

Azote gas.

22d. I next proceeded to the examination of azote gas. I began by examining that obtained from atmoſpheric air, in which a candle was extinguiſhed.

The abſorption was ſix inches.

23d. I examined that obtained by the combuſtion of ſulphur, and had an abſorption of ſeven inches and three lines: it is to be obſerved that this gas always contains ſulphureous acid, and for that reaſon more of it was abſorbed.

A ſecond experiment with the charcoal gave me ſeven inches and two lines.

I afterwards tried the azote gas obtained from atmoſpheric air in which I had extinguiſhed a large piece of charcoal under a bell: the abſorption was ſeven inches ſix lines; but this gas always contains a little carbonic acid, and conſequently there is a larger abſorption.

24th. To be ſure of having the pureſt azote gas without mixture, I took the gas obtained by decompoſing nitrous acid by means of hidrogen gas, which laſt gas had been obtained from the decompoſition of water by iron.

The abſorption was only ſix inches and one line.

We have not yet very correct ideas with reſpect to azote: it appeared to me, however, in ſeveral of the experiments which I made with atmoſpheric air, that when it is united with aqueous vapour, more of it is abſorbed than when in a dry ſtate. Beſides it is evident that the azote gas is not much abſorbed, which correſponds with my firſt experiments.

(The Concluſion in our next.)

XI.

On the Commerce of Hens Eggs, and on their Preſervation. By M. PARMENTIER [*].

The bulk of the egg does not depend ſo much on the nouriſhment as on the breed of the hen.

THERE are very conſiderable differences in the eggs of hens in reſpect to ſize. Some are as large as the eggs of ducks, while others reſemble thoſe of pigeons. M. Parmentier, from obſervations on a great number of hens of different breeds, which he reared in the ſame place, has found that the bulk of the eggs depends much more on the breed of the hen than on the quantity of nouriſhment. Thoſe breeds which give the largeſt eggs are not however to be preferred on that account, becauſe with them as much may be loſt in the quantity of eggs as is gained by their ſize. Of all the breeds known in France the author gives the preference, with reſpect to the produce of eggs, to that which is called the common hen, and which is only common becauſe its merit is known. Thoſe which have black legs are in greater eſteem than thoſe with yellow ones.

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[*] From Bulletin des Sciences, Tom. III. p. 214.

From comparative experiments continued for a year, M. Parmentier found that though the eggs of this breed were not ſo large as ſome others, yet every thing beſides being equal they produced at leaſt one half more.

Superior breed

Next to this breed of hens comes the creſted hen and the large Flemiſh hen. The one is more delicate eating, becauſe laying leſs than the common hen, it grows fatter; the other, without being more productive, is preferable for raiſing chickens. The *ſilky* hen *(poule de ſoie)* ſo beautiful in its form and in the fineneſs of its plumage, ſo careful in laying, ſo aſſiduous in ſetting, ſo tender of its chicks, might be recommended, but unfortunately two of its eggs are not worth one common egg. This circumſtance places it among thoſe which muſt be left for the curious.

Management the hens.

After the choice of the breeds, care muſt be taken that the hens are neither fed too abundantly nor too ſparingly; that they do not wet their feet, that they are ſufficiently cloſe in the rooſt to heat and electrify each other; and, that they meet with a little warm dung in the day time.

Unfecundated eggs leaſt liabl to ſpoil.

When the only object in keeping hens is to procure eggs, and thus turn to profit the grain which remains among the chaff and in the dung-hills, it is wholly uſeleſs to keep cocks at the ſame time, becauſe experience has ſhown that hens deprived of the male do not lay leſs than thoſe which have it. The ſaving by this is not the only nor the greateſt advantage. The unfecundated eggs keep much better than thoſe which are fecundated. It has been found by experiment that they can ſupport a heat of 32° (87° F.) for thirty or forty days without experiencing any change. It is therefore evident that the evaporation of the liquors is not, as Reaumur thought, the immediate cauſe of the putrid change in eggs, and that to preſerve them, it is not ſufficient to cover them with greaſe or oil, as this learned man recommends, ſince in the experiment mentioned above the unfecundated eggs did not ſpoil, though they loſt conſiderably by evaporation. The fecundation, by the principle of life which it communicates to the germ, expoſes the eggs to many accidents which do not take place with thoſe in which the male has had no ſhare.

Cauſes of the ſpoiling of eggs.

M. Parmentier particularizes ſome of theſe accidents. Some of them ariſe from the commencement of the developement of the germ. Sometimes it is enough if ſeveral hens lay their eggs in the ſame neſt; for the egg which is firſt laid in it, partaking in ſucceſſion, and for ſome hours, of the heat of the hens which follow each other, undergoes a ſpecies of incubation which excites the vitality of the germ, and this egg becomes changed, though it is but recently laid. It is thus that eggs of the ſame age appear frequently to differ in their freſhneſs. At other times the change in the egg may ariſe from the fecundating germ being killed, either by thunder, or in the conveyance, by the jolting of the carriage or the rolling of the veſſel, or by the lapſe of time. When the germ is once dead it corrupts, and communicates the corruption to that which ſurrounds it. This theory appears to explain one method I made uſe of ſucceſsfully for preſerving eggs even when fecundated: it conſiſts in plunging them for a couple of ſeconds into boiling water. It is known that by this means they become ſuſceptible of being preſerved for ſeveral months, if they are afterwards kept in a cool place, or in ſalt. M. Parmentier ſuſpects that the utility of this proceſs depends on the deſtruction of the vitality of the germ by the boiling water.

Utility of immerſing the eggs for a ſhort time in boiling water.

Eggs laid at ſea keep longer than others.

Mariners aſſert that eggs laid at ſea keep better than others. May not this ariſe from the hens on board a ſhip having no communication with cocks? For the ſame reaſon the diminiſhed vigour of the cocks in our poultry yards, in autumn, may be one cauſe of the eggs laid in that ſeaſon being more capable of being kept than thoſe of the firſt laying, to which may be added, that the hens are at that time fed more with grain and leſs with herbage.

Requiſites for keeping eggs freſh.

From theſe obſervations, M. Parmentier thinks that the firſt condition towards obtaining eggs capable of being preſerved and tranſported without ſpoiling, is not to keep a cock with the hens in the poultry yard. It is an error to ſuppoſe that eggs not fecundated have a worſe taſte than thoſe which are ſo. The author aſcertained that the moſt delicate palate was incapable of diſcovering any difference. Afterwards it is only requiſite to ſhelter the eggs from humidity, light, heat, and froſt. The method which ſucceeded beſt with the author was to procure baſkets made of ſtraw, in which he placed the eggs with layers of chaff between them. The ſtraw and the chaff are dry materials, ſmooth, very bad conductors of heat, and conſequently, very proper to preſerve the character of freſhneſs in the eggs: theſe baſkets were afterwards ſuſpended in a dry, dark and airy ſituation.

XII.

Method of giving the Colour, Grain, and Hardneſs of Steel to Copper. By B. G. Sage*.

The readieſt and moſt certain method of uniting copper and phoſphorus.

MARGRAFF and Pelletier have publiſhed their inveſtigations relative to the union of phoſphorus with different metallic ſubſtances; the French chemiſt has brought this proceſs to perfection: it was by repeating and varying his experiments that I diſcovered that the readieſt and moſt certain method of phoſphorating copper is to take the copper in the metallic form, to melt it with two parts of animal glaſs, and a twelfth part of powdered charcoal. But it is eſſential that the copper ſhould offer a large ſurface; an advantage which is obtained by uſing chips of this metal, which are to be placed in alternate layers with the animal glaſs mixed with the powdered charcoal. I expoſe the crucible to a fire briſk enough to melt the animal glaſs; it forms phoſphorus; the greateſt part of which burns, while another part combines with the copper, in which it is ſo conſolidated that it does not quit it, although it is kept in fuſion for twenty minutes under the animal glaſs which has not been decompoſed.

Phoſphorated copper.

When the crucible has cooled and is broken, the phoſphorated copper is found under the glaſs, which has paſſed to the ſtate of a red enamel, in the form of a grey and brilliant button: on weighing it, it is found to have acquired one twelfth by this operation.

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* From Journal de Phyſique, Meſſidor, An. XII.

Its action on poliſhed iron.

If the melted phoſphorated copper falls on a plate of poliſhed iron, it ſpreads itſelf in the form of plates of various configurations which are irideſcent like the neck of a pigeon.

Is more fuſible than copper.

Phoſphorated copper is much more fuſible than red copper: it may be often melted under charcoal powder without loſing its properties.

Does not quit the phoſphorus without difficulty.

The ſame phoſphorated copper, expoſed for a long time under a muffle, does not ſeparate from the phoſphorus without great difficulty.

Reſembles ſteel in hardneſs, grain and colour, and does not tarniſh by expoſure to the air.

The copper thus combined with the phoſphorus acquires the hardneſs of ſteel, of which it has the grain and the colour; like it, it is ſuſceptible of the moſt beautiful poliſh; it is turned eaſily in the lathe; it does not change in the air: I have for fifteen years, kept buttons of poliſhed phoſphorated copper in my laboratory, which have not experienced the leaſt change. The copper does not develope any ſmell on being rubbed: if it was ductile it would be of the greateſt utility, ſince fat bodies do not ſeem to make any impreſſion upon it.

A ſmall quantity of charcoal to be uſed in its preparation.

In the phoſphoration of copper there is only a part of the animal glaſs decompoſed: becauſe a ſufficient quantity of charcoal is not employed to convert all the acid into phoſphorus; but it is neceſſary that this ſhould be the caſe, that the phoſphorated copper may ſeparate and collect with facility.

Red enamel.

The deep red enamel which is formed in this experiment may be advantageouſly employed for porcelain or the enamels, ſince it does not change colour in the fire.

Copper and phoſphorus can only be combined in the dry way.

Copper cannot combine with phoſphorus except in the dry way. If a cylinder of phoſphorus is put into a ſolution of nitrate of copper diluted with four or five thouſand parts of water, at the end of eight days the copper will be found in a metallic form, cryſtallized and ductile, forming a caſe for the cylinder of phoſphorus.

Obſervations

XIII.

Obſervations on Mr. GOUGH's *two Letters on Mixed Gaſes.*[*]

To Mr. NICHOLSON.

SIR,

Introduction.

MY reply to Mr. Gough's ſtrictures being chargeable, it ſeems, with acrimony and ridicule, and with having but few arguments, and thoſe in appearance negligently conducted. (though the laſt charge requires a whole letter to make it good); I propoſe, in what follows, to avoid the two former of theſe as much as may be, and to be as careful as poſſible in conducting my arguments; ſo, that if they appear to Mr. G. deſtitute of that logical preciſion which characteriſes his, he may aſcribe it to my inability, and not to any want of inclination.

Obſervations on Mr. Gough's letter on mixed gaſſes.

The accuracy of Mr. G.'s demonſtration in the former letter, depends upon that of three phyſical data: *1ſt*, The ſpecific gravity of azotic gas; *2d*, The ſpecific gravity of oxigenous gas; and, *3d*, The quantity of oxigen in a given volume of atmoſpheric air. If any *one* of theſe be wrong, it may prove fatal to his demonſtration: now it unfortunately happens that all *three* are wrong, and that, when corrected, they prove the very reverſe of his propoſition; namely, that *atmoſpherical air is a mechanical mixture of oxigenous and azotic gaſes.*

Mr. G.'s data are,

Sp. gravity of azotic gas, .985
——————- oxigenous gas, 1.103
Quant. of oxigen in atmoſphere, *per cent.* in bulk, 22-25-28, uncertain which.

The true, or at leaſt the moſt approximate numbers, are,

Sp. gravity of azotic gas, - .966
——————— oxigenous gas, 1.127
Quant. of oxig. in atmoſphere, *per cent.* in bulk, - - 21

Then, *per* Mr. G.'s theorem, 21 × 1,127 = 23.667
and 79 × .966 = 76.314
———
99.981

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[*] See Pages 107, 160, of the preſent Vol.

The

Obſervations on Mr. Gough's letter on mixed gaſes.

The ſum is ſo near 100, that Mr. G. will not venture to preſume any thing on the difference, further than that the data are ſtill in a ſmall degree incorrect, which I believe no body will diſpute with him.

Mr. G. by this time is ready to query, How do you know your data to be more correct than mine? I will now inform him.

Dr. Prieſtley was perhaps the firſt to inveſtigate the ſpecific gravities of the two gaſes in queſtion. His method was very exceptionable; but ſuch is generally the caſe in the infancy of any ſcience. *(See Vol. II. Page* 452, *abridg. Ed. of his Nat. Philoſ.)* He found azotic gas as much lighter than common air as oxigenous was heavier. Mr. Kirwan ſoon after gave a much nearer approximation; namely, the one which Mr. G. has adopted. Lavoiſier alſo found the ſpecific gravity of ſeveral gaſes. *(See Elements of Chemiſtry, Append. Table* 7). His reſults nearly agree with Kirwan's in regard to oxigen, but differ conſiderably in regard to azote. Laſtly, Mr. Davy, when inveſtigating the compounds of azote and oxigen, found it expedient to aſcertain with preciſion the ſpecific gravity of the two gaſes. Having every means [illegible] his predeceſſors, and their reſults before him, he ought [illegible] leaſt to have decided in regard to the difference between them. Accordingly he finds his reſults to agree with Lavoiſier's in reſpect to azote; but he finds the ſpecific gravity of oxigen ſomewhat greater than either of them. *(See Reſearches, pag.* 565).

The following table exhibits the reſults of all theſe together, reduced to the ſtandard of atmoſpheric air.

Specific Gravities of

		Azotic Gas.		Atmoſ. Air.		Oxigenous Gas.
According to	Prieſtley,	.989	—	1.000	—	1.011
	Kirwan,	.985	—	1.000	—	1.103
	Lavoiſier,	.966	—	1.000	—	1.102
	Davy,	.966	—	1.000	—	1.127

As for Mr. G.'s third datum, I think it ſcarcely pardonable in the preſent day, for any one to pretend to diſcuſs a queſtion concerning the conſtitution of the atmoſphere, under the uncertainty whether it contains 22 or 28 *per cent.* of oxigen. He ought to be acquainted with the hiſtory of eudiometry, and

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and to repeat experiments conſidered as deciſive, eſpecially as they are of a ſimple nature. All the chemiſts of Europe ſeem to be agreed, that either 21 or 22 *per cent.* in bulk, is the proper number. My own experience gives 21 for the neareſt integer, which agrees with Mr. Davy's in your Journal, (quarto Series), Vol. V. page 175: if Mr. G. entertains any doubt on the ſubject, I would refer him to that paper, and he will receive ample ſatisfaction.

After what has been ſaid, I think Mr. G. muſt be ſatisfied that he has virtually demonſtrated the atmoſphere to be a mechanical mixture of the two gaſes: If, however, he ſtill alledge that I only oppoſe one authority to another, and that his is as good as mine; then I would recommend him to ſatisfy himſelf as follows: mix 21 parts (or a quantity of gas containing 21) of pure oxigen, and 79 of pure azote, together; after this proceed to the analyſis of the gaſes, and examine the differences between the reſults, and thoſe done from the analyſis of 100 parts of atmoſpheric air.

I cannot conclude this article without obſerving, that Mr. G. muſt have been totally unacquainted with the opinions of chemical philoſophers on the ſubject, or he would not have expanded into the compaſs of five or ſix pages a ſimple argument, which has often been adverted to by others, and is now wholly abandoned as untenable. Mr. Davy, in his Reſearches, publiſhed in 1800, advocating the notion of atmoſpherical air being a chemical compound, produces four evidences, one of which is ſtated as follows: *See Page* 326.

" *2dly*, The difference between the ſp. gr. of atmoſpheric air, and a mixture of 27 parts oxigen and 73 nitrogen, as found by calculation; a difference apparently owing to expanſion in conſequence of combination." In a note he adds, " The two firſt evidences have been often noticed." This gentleman however ſoon after, finding that the atmoſphere contained only 21 *per cent.* of oxigen, muſt have ſeen that this evidence was not to be admitted. Since that time it has not been urged by any one to my knowledge.

I come next to Mr. G.'s reply at page 160. This at the commencement purports to be a defence of the charge, that *my arguments are but few; and negligently conducted;* at the concluſion it is aſſerted to be an *anſwer to all my objections, and ſomething more.* This language may be that of logical preciſion

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preciſion with Mr. G.; but it would have been more intelligible to me if he had ſtated the objects of his reply thus:

1ſt, A refutation of Mr. D.'s arguments in favour of his ſyſtem.

2d, An anſwer to his objections brought againſt mine.

3d, New objections to his ſyſtem.

The firſt thing worthy of notice is the objection to my argument for the mutual penetrability of gaſes. I have aſſumed *one* poſtulate and taken *two,* and the latter of them is erroneous, namely, that *all gaſes are porous.* It is true, I have taken two poſtulates into the argument, without expreſsly requiring both; the former being peculiar to my theory, was neceſſarily demanded in a formal way; the latter being the reſult of all experience, and never in any one inſtance having been found to fail, I thought it might *tacitly* be aſſumed. However, the judgment of philoſophers muſt be ſuſpended on this head, as Mr. G. it ſeems, is about to prove that *no gas is porous,* and that *a cubic foot of one gas cannot be put into a veſſel that is previouſly occupied by another gas.* Mr. G. ſurely cannot be ſerious in this objection; but merely uſes it to gain time, and means to turn it off with a laugh, that he has at leaſt produced one *ſolid* argument againſt my *airy* hypotheſis.

Mr. G. finds it extremely convenient for his purpoſe, that I ſhould grant him the following poſtulate: "If a particle of vapour can paſs freely through the air, a ſecond can alſo ſucceed it at any given diſtance." I certainly cannot concede ſo indefinite a demand; but it will perhaps be of equal uſe to him to have the following: If d be the diſtance of two particles of vapour of the temperature of 212°, and preſſure 30 inches; then, at the temperature of 60° or upwards, if one particle of vapour can paſs freely through the air, a ſecond may ſucceed it at any diſtance greater than $4\,d$.

As for the important argument which I conſider equivalent to a demonſtration of the nature of vapour, and of its relation to gaſes, and entirely inimical to the notion of chemical affinity, Mr. G. has not ventured to revive it: Probably he has ſomething in reſerve on this head. I mean the argument derived from the fact, that a vacuum, or any kind of air of any denſity whatever, takes up juſt the ſame quantity of any vapour.

The

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The propoſition reſpecting the mechanical action of air on the ſurface of water, will be anſwered by Mr. G. when he has proved that *air has no pores*, or no *capacity for the reception of water*. For, nothing can be more clear than that, whatever may be the preſſure of the atmoſphere, and however few the points of its action, water cannot be forced into the pores of air, if it have none.

Mr. G. proceeds to ſtate two new facts, which are ſaid to be inexplicable on my principles: I muſt undertake the arduous taſk. The facts are, *1ſt*, Air containing aqueous vapour is ſpecifically lighter than air without it, *cæteris paribus*; and, *2d*, A bottle internally moiſt, containing air, being heated, more air is expelled than if the bottle had been dry. Both granted.—Now for the explication. The ſpecific gravity of aqueous vapour has been found by De Sauſſure, Watt, and others, to be about $\frac{2}{3}$ or $\frac{3}{4}$ of that of atmoſpheric air in like circumſtances; by ſome experiments of my own I am induced to think it is nearly .7, that of air being 1. Let the temperature be 64°, and the air be filled with vapour as much as poſſible in the temperature, in which caſe $\frac{1}{50}$ of the elaſtic force will be due to the vapour. (*See Manch. Mem. Vol. V, P. 2, page* 559); then, by the theorem ſo elaborately exemplified in Mr. G.'s former letters, we have $\frac{49 \times 1 + 1 \times .7}{50} = \frac{49.7}{50} = .994$, for the ſpecific gravity of common air filled with vapour at 64°, when that of dry air of the ſame temperature would be 1. Thus it appears that my hypotheſis not only explains the fact of diminiſhed ſpecific gravity, but accounts for the quantity of diminution. Can Mr. G.'s theory of chemical ſolution do this?

In the ſecond caſe we find heat generating vapour, which increaſing in quantity and force with the temperature, diffuſes itſelf through the air in the bottle; the hand being occaſionally removed from the mouth, ſuffered the extra-portion of air and vapour to ſtart out; juſt as if there had been a generation of a like portion of oxigen, or any other permanent gas, inſtead of the vapour; in which caſe a portion of both does certainly eſcape. Mr. G. finds the vapour generated this way between 59 and 126°, to be rather leſs than $\frac{1}{9}$ of at-

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moſpheric force; it ought to be juſt $\frac{1}{9}$ by the table above referred to. But it is obvious, that by removing the hand occaſionally, the *full* quantity of vapour for any temperature could never be obtained in this way, no more than pure oxigen could be procured in the bottle by a ſimilar proceſs. The grand queſtion with Mr. G. however, is, How does vapour of half an inch force, expand the pores of air ſubject to 30 inches of preſſure? And his anſwer ſeems to be, it is impoſſible, according to the axioms of dynamics. This queſtion is what I ſhall now conſider.

Having myſelf ſtudied the principles of dynamics, as well as thoſe of many other mathematical and phyſical ſciences, under the tuition of Mr. Gough, I feel under ſtrong obligation to him; but theſe, he will readily grant, do not bind me to ſubſcribe to his opinions, when I cannot perceive them to be well founded. He charges me, in the preſent inſtance, with a miſtake in regard to dynamics; but he has not pointed out any particular axiom which I have offended: The miſtake, I think, is with him, and ſhall endeavour, in what follows, to point it out.

It is a principle in dynamics, that whenever a ſyſtem of bodies act upon each other, and are in a ſtate of equilibrium, the *leaſt* force impreſſed upon any one diſturbs the equilibrium. Thus, the ocean and the air, though bound to the earth by its ſuperior attraction, are nevertheleſs diſturbed by the feebler influence of the moon. Air in a bottle is a ſyſtem of particles at equal diſtances repelling each other, but in equilibrium by the gravity of the incumbent atmoſphere; conſequently the *leaſt* force impreſſed upon them muſt diſturb that equilibrium. Now, *ex hypotheſi*, air does not repel vapour at a diſtance, but only in contact; therefore vapour can be formed in ſuch a ſyſtem: when once formed, it meets with no elaſtic reſiſtance or repulſion but from particles of its own kind; therefore it is conſtantly tending outwards, where the particles of its own kind are leaſt denſe: in its way it infringes upon particles of air, and exerts ſuch force as it is capable of upon them: a number of particles of air are thus gently propelled in the direction of the ſtream, and the reſt of the ſyſtem are obliged to accommodate themſelves in order to preſerve the equilibrium: thus the diſtances of the particles of air are gradually increaſed, and that in proportion to the force which the vapour exerts. Therefore vapour of the leaſt poſſible force can, in ſuch circumſtances, extend the pores of air. Q. E. D.

I remain your's,

J. DALTON.

Mancheſter, Nov. 15, 1804.

XIV.

Some Account of a Condenſer of Forces, or a Method of obtaining the greateſt poſſible Effect from a firſt Mover, of which the Energy is ſubject to Increaſe or Diminution within certain Limits; and in general to vary at Pleaſure the Reſiſtance to which the Effort of the firſt Mover forms an Equilibrium in any Machine whatever, without changing any Part of the Conſtruction. By R. PRONY.*

Problems in mechanics,

THE problem of mechanics, of which the ſolution is here given, is one of the ſmall number of thoſe which, leading to reſults independent of the particular mechaniſm of the machine to which they are applied, preſent, in their ſolution, a generality which may be compared with that of the rational mechanics, or analyſis.

It may be enunciated in the following terms:

enunciated.

" Any machine being conſtructed, to find, without making any change in the conſtruction, a means of tranſmitting to it the action of the firſt mover, by fulfilling the following conditions; *viz.*

Conditions.

" 1. That it may be poſſible at pleaſure, and with great ſpeed and facility, to vary the reſiſtance (againſt which the effort of the firſt mover muſt continually make an equilibrium) in limits of any required extent.

" 2. That the reſiſtance being once regulated, ſhall be rigorouſly conſtant until the moment when it is thought proper to increaſe or diminiſh the ſame.

" 3. That in the moſt ſudden variations of which the effort of the firſt mover may be capable, the variation in velocity of the machine ſhall never undergo a ſolution of continuity."

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* From the Bulletin of the Philomathic Society at Paris, No. 83.

Solution exhibited in an engine.

I ſhall here apply the ſolution which I have diſcovered of this problem, to the dynamic effect of wind; it will be eaſy to make the ſame general when the other firſt movers are uſed.

Problem in dynamics: To oppoſe a conſtant weight to a variable firſt mover, and tranſmit the force, &c.

The ſection and plan of the machine are exhibited in Plates XV. and XVI. O O repreſents the vertical arbor to which windmill ſails are adapted; *e e e e* is an aſſemblage of carpentry, of which one of the radii O *e*, bears a curved piece *b d*, of iron or ſteel: Vertical axes of rotation *a a a*, being placed round the axis O O, alſo divide the circumference in which they are found into equal parts.

Each of theſe axes carries a curve *a f*, of iron, ſteel, or copper; ſo ſituated, that when the wind acts upon the ſails, the curve *b d* preſſes againſt one of the curves *a f*, and cauſes the vertical axis to which this laſt curve is fixed, to make a portion of a revolution.

The curves *b d* and *a f* muſt be ſo diſpoſed, that when *b d* ceaſes to preſs on one of the curves *a f*, it ſhall at the ſame inſtant begin to act upon the following curve: the number of axes which are provided with theſe curves, muſt be determined by the particular circumſtances of each caſe; and it is alſo practicable to ſubſtitute, inſtead of *b d*, a portion of a toothed wheel having its centre in the axis O O, and to place portions of pinions inſtead of the curves *a f*, but the diſpoſitions repreſented in the figure are preferable.

Each of the axes *a a a a* (which are all fitted up alike, though, for the ſake of clearneſs, only one of them has its apparatus repreſented in the drawing), each of theſe carries a drum or pulley *t t r r*, on which is wound a cord that paſſes over a pulley *p*, and ſerves to ſupport a weight Q by means of the lever F G, upon which this weight may be ſlided and faſtened at different diſtances from the point of motion G.

The ſame axes *a a* paſs through the pinions *q q*, to which they are not fixed; but theſe pinions carry clicks or ratchetts, which bear againſt the teeth *r r*; ſo that, when the weight Q tends to riſe, the ratchett gives way, and no other effect is produced on the pinion *q q*, either by the motion of the axis or of the drum *t t r r*, excepting that which cauſes the aſcent of the weight *q q*. But the inſtant that the curve or tooth *b d* ceaſes to bear againſt one of the curves *a f*, after having cauſed the correſponding weight Q to riſe, that weight Q tends to redeſcend, and then the toothed wheel *r r* acts againſt the

Problem in d namics: To c poſe a conſtan weight to a va riable firſt mover, and tranſmit the force, &c.

the ratchett, ſo that Q cannot deſcend without turning the pinion *q q* along with the drum *t t r r*.

The pinion *q q* takes in the wheel *a b*, from the motion of which the uſeful effect of the machine immediately reſults; ſo that the effect of the deſcent of one of the weights Q, is to ſolicit the wheel A B to motion, or to continue the motion in concurrence with all the other weights Q, which deſcend at the ſame time. This wheel A B carries beneath it oblique or bevelled teeth G D, which take in a like wheel C E, and cauſe the buckets at S to riſe.

The alternation in the motion of theſe buckets may be effected by the mechaniſm I have deſcribed in the firſt volume of the Memoirs of the Inſtitute.

From the preceding deſcription it is ſeen that the machine, being ſuppoſed to ſtart from a ſtate of repoſe, the wind will at firſt raiſe a number of weights Q, ſufficient to put the machine into motion, and will continue to raiſe new weights while thoſe before raiſed are fallen; ſo that the motion once impreſſed will be continued.

Among the numerous advantages of this new mechaniſm we may remark the following:

1. No violent ſhock can take place in any part of the mechaniſm.

2. The uſeful effect being proportioned to the number of weights Q, which deſcend at the ſame time, this effect will increaſe in proportion as the wind becomes ſtronger, and cauſes the ſails to turn with more velocity.

3. The weights Q being moveable along the levers F G, it will always be very eaſy to place them in ſuch a manner as to obtain that ratio of the effort of the firſt mover to the reſiſtance, as ſhall produce the maximum of effect.

4. From this property it reſults, that advantage may be taken of the weakeſt breezes of wind, and to obtain a certain product in circumſtances under which all other windmills are in a ſtate of abſolute inactivity: this advantage is of great importance, particularly with regard to agriculture: the windmills employed for watering lands are ſometimes inactive for ſeveral days, and this inconvenience is more particularly felt in times of drought. A machine capable of moving with the ſlighteſt breeze, muſt therefore offer the moſt valuable advantages.

I ſhall give a more ample account of this apparatus in a memoir which I ſhall preſent to the Inſtitute, as ſoon as the machine which I am now erecting in the country ſhall be completed.*

XV.

Abſtract of a Memoir on the Poſſibility of obtaining Pruſſiate of Potaſh free from Iron; the Unalterability of the Pruſſic Acid at high Temperatures; and the true Nature of the Combinations of this Acid with different Baſes.† By BUCHOLZ.

Utility and advantages of pruſſic compounds.

THE combination of pruſſic acid with ſalefiable baſes, forms a claſs of ſalts of the greateſt utility to the analytical chemiſt; for by means of them he is not only enabled to aſcertain whether a metallic ſubſtance be preſent in any ſolution which forms the ſubject of his reſearch, but alſo what metal is preſent, as well as its reſpective quantity. But in order to be accurate in this reſpect, the re-agent employed muſt itſelf be free from metallic admixture; or the quantity and nature of the metal it contains muſt at leaſt be known. To accompliſh this, chemiſts have hitherto laboured in vain. To remedy theſe defects, Mr. Bucholz has inſtituted a number of experiments which led to facts hitherto unknown; and as they are highly important, we ſhall exhibit the reſults of the principal ones, which are as follows:

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* The practical mechanic may perhaps find it an advantage to be informed, that the whole effort of a firſt mover cannot be tranſmitted by this, or any other method of raiſing weights, in order to operate by their fall. If the wind had been employed to raiſe a maximum of weight through a given ſpace in a given time, this weight would be leſs than would continue in equilibrio at reſt againſt the ſame force, as is well known; and if this weight be again employed in like manner to raiſe another maximum of weight, this alſo will be leſs than the former, &c. For this reaſon it is that the fly has been uſed as an equalizer of ſteam-engines, in preference to the older method of raiſing water and ſuffering it to deſcend on an overſhot wheel. W. N.

† Abridged from a memoir in Gehlen's new Journal of Chemiſtry, Vol. I. Part IV. Page 406, by — A.

1. Pruſſic acid can only be formed during the carbonization of blood or animal matter, *at a red heat*: We, therefore, need not be afraid of heating the mixture of blood and alkali, in the preparation of pruſſiated alkalies, to incandeſcence.

Pruſſic acid is formed only at red heat.

2. The affinity of pruſſic acid with alkalies is greater at high temperatures, and in the *dry way*, than at low temperatures, and in the *humid way*.

Its affinity with alkalies is then greateſt.

This obſervation diſtinguiſhes pruſſic acid from the reſt of the ſo called animal and vegetable acids, and their combinations with alkaline baſes; for all of them are deſtroyed at a red heat. Hence it is obvious,

All other vegetable acids are deſtroyed by ignition.

3. That in the preparation of pruſſiated alkalies, the preſence of water ſhould be avoided as much as poſſible.

Water ſhould be avoided.

4. The direct combination of pruſſic acid with alkalies, cannot be accompliſhed.

P. acid and alk. do not directly combine.

5. Pure pruſſiated alkalies are decompoſable by the affuſion of water; part of the pruſſic acid eſcapes, and may eaſily be recogniſed by the odour of bitter almonds, and frequently alſo by that of ammonia.

P. pruſſiated alk. decompoſable by water.

6. The combination of pruſſic acid with alkalies can only be effected *at all temperatures* up to a red-heat, by the interpoſition of a portion of oxide of iron; and the affinities conſiſting between the pruſſic acid and alkali, are retained with a greater force in the humid way, in the ratio of the ponderable quantity of iron preſent.

P. acid does not combine with alk. at low temp. but by medium of iron, &c.

7. All the precipitates obtained in chemical analyſis by means of pruſſiated alkalies prepared in the uſual manner, contain more or leſs iron, without exception; whereas the ſame precipitates, produced by the action of abſolutely pure pruſſiates, are free from that metal, and of a different colour than the former.

Common pruſſiates contaminate metals with iron; pure p. give a different reſult.

8. The affinity of oxide of iron to charcoal, is more powerful than the joint attraction of pruſſic acid and potaſh to that metal; hence we always find that the greateſt quantity of that metal remains behind with the charcoal, in the uſual proceſs for obtaining pruſſiate of potaſh.

Moſt of the iron preſent remains in the coal, when p. alk. is made.

9. The precipitability of the oxides of metals by pruſſiated alkalies, is in the ratio of their oxidability, or quantity of oxigen they contain.

Metals are precip. by p. alk. in the order of oxigenation.

Such are the obſervations of this chemiſt. Other leſs intereſting facts will be omitted in the preſent abſtract.

Experimental

Experimental Enquiries concerning the beſt Method of preparing Pruſſiate of Potaſh free from Iron.

Preparation of pruſſiate of alkali, pure. *Low ignition* of dried blood and potaſh, and ſolution by water.

Four oz. dried blood were intimately mixt with a ſolution of potaſh (containing one ounce of potaſh), then evaporated to dryneſs, ignited to redneſs, till no more flame undulated at the ſurface of the ignited maſs. The ignited maſs was diffuſed through ſix ounces of water, and the ſolution filtered. The filtered fluid was colourleſs, it contained an exceſs of alkali, and emitted a ſtrong odour of bitter almonds. It yielded, on being evaporated as expeditiouſly as poſſible, a ſaline maſs, conſiſting of a mixture of pruſſiate and ſub-carbonate of potaſh. In order to ſeparate theſe two ſalts, one drachm of the ſaline maſs was introduced into a vial containing a mixture of half an ounce of highly concentrated alcohol and half a drachm of acetic acid, of 1,056 ſpec. grav. On agitating the fluid no ſenſible effervеſcence took place, but much pruſſic acid was diſengaged; a proof the pruſſic acid was retained with a leſs affinity by the alkali than the carbonic acid. On examining the reſidue, which had been treated with acetic acid and alcohol, it was found to contain only a ſmall portion of pruſſiated alkali.

The ſalt was treated with acetic acid and alcohol.

Very little pruſſiate, and not ſeparable from the carbonate by thoſe agents.

From this experiment we learn, that one part of potaſh cannot be converted into pruſſiate of potaſh, by being heated to redneſs with four of blood; and that the quantity of either free or carbonated alkali, under theſe circumſtances, cannot be ſeparated from the pruſſiated potaſh by means of acetic acid and alcohol.

Leſs of blood was then uſed in the ignition.

In order to learn if a leſs quantity of blood would not be more advantageous for the production of pure pruſſiate of potaſh, four ounces of dried blood, and two of carbonate of potaſh, were heated to redneſs in a crucible till no more flame appeared. On covering the mixture with charcoal powder, and again heating it, a prodigious evolution of ammonia took place; a phenomenon I do not venture to explain. The maſs, after having been diffuſed through water, filtered and evaporated, yielded a crop of cryſtals, conſiſting of pruſſiate and ſub-carbonate of potaſh, the former predominating conſiderably.

Great evolution of ammonia.

The ſubcarbonate and pruſſiate not ſeparable by alcohol and acetic acid.

On ſubjecting this mixture of ſalts to the joint action of acetic acid and alcohol, it was found impoſſible to ſeparate the pruſſiate of potaſh from the ſub-carbonate, Being thus perſuaded that a leſs quantity of blood was not more advantageous for forming pure pruſſiate of potaſh, I again mingled two ounces of dried blood with one of carbonate of potaſh: this mixture was treated as before, with the exception that the maſs, after the flame had entirely diſappeared, was ſtrongly ignited for three quarters of an hour. The obtained maſs, after refrigeration, weighed nine drachms. Digeſted cold with four ounces of water, and filtered and evaporated, it afforded *a dark coloured* fluid. On dropping into a conſiderable quantity of muriatic acid, no blue, but a white precipitate fell down, which was inſoluble in muriatic acid: Acetic acid occaſioned no change in this dark-coloured fluid; and on mingling it with ſulphuric acid, and evaporating the mixture to dryneſs, and re-diſſolving the maſs in water, it yielded a ſmall quantity of pruſſian blue; a proof that iron was preſent in this fluid.

Blood and alkali, as in the firſt experiment, *ſtrongly ignited* for three quarters of an hour.

The fluid of ſolution was dark, and contained ſome iron, though very little.

The quantity of iron being very ſmall, but the colour of the fluid very dark when compared with thoſe of the former proceſſes, it was natural to ſuppoſe, that the colouring-matter could not be attributed to the minute quantity of iron preſent, but that perhaps a portion of charcoal was diſſolved in the fluid: to inveſtigate which the following experiments were inſtituted:

Repetition of the experiment with fuſion of the maſs.

A like quantity and like proportions of blood and carbonate of potaſh, as ſtated laſt, were gradually heated to incandeſcence, and the fire gradually augmented, *until the maſs began to fuſe* on the ſides of the crucible. The maſs, after having been diffuſed through water and filtered, yielded a much darker-coloured fluid, which, when mingled with muriatic acid, yielded a pearl-coloured precipitate. After being mixt with muriatic or ſulphuric acid, evaporated and rediſſolved in water, it afforded a conſiderable quantity of oxide of iron; a proof that the union of the pruſſic acid with potaſh is permanent at very high temperatures, but that this combination, under ſuch circumſtances, exerciſes a ſtrong action upon the oxide of iron contained in the blood.

The pruſſic acid remains united with alk. at very high temp. and takes iron ſtrongly.

Theſe facts were proved by repeating this experiment, but taking care to expoſe the maſs to a ſomewhat leſs degree of heat. The fluid now obtained was leſs coloured, and yielded leſs oxide of iron.

Leſs oxide taken up by leſs heat.

This

Experiments with pure alkali.

This being proved, the author varied his experiments, ſo as to be thoroughly convinced of the facts. He alſo endeavoured to aſcertain, whether alkalies, freed from carbonic acid, were better calculated for the production of pruſſiated alkali; and, if poſſible, to find out the proper proportions of ingredients for obtaining this ſalt. With that view, a quantity of ſolution of potaſh, containing one ounce of alkali freed from carbonic acid, was mixt with three ounces of blood, and evaporated to dryneſs. (It was of a colophony colour, ſoluble in water, and emitted, on being heated, a very ſtrong odour of ammonia). On being transferred into a crucible, and gradually heated till no more flame appeared, and diffuſed through four ounces of water, it yielded a *limpid* fluid, of a ſtrong alkaline taſte, and odour of bitter almonds. The uſual experiments proved, that it contained comparatively little pruſſiated alkali, but a large quantity of carbonate of potaſh.

Low heat produced little combination of p. acid and the alkali.

Exp. to determine the proportions of carbonate and blood requiſite to produce the pruſſiate, &c.

Being thus convinced that this experiment proved fruitleſs, it was deemed neceſſary to aſcertain the reſpective quantity of oxide of iron contained in the pruſſiated alkali, that might be produced from a given quantity of blood and carbonate of potaſh.

The experiments with low heat.

4 oz. blood and 1 alk. carb.

With that view, four ounces of dried blood were mixt with a ſolution of carbonate of potaſh, containing one ounce of carbonated alkali: the mixture, after being evaporated to dryneſs, was heated till the flame ceaſed to appear. It now weighed 1½ oz. On being elixiviated with eight ounces of water, it afforded a fluid of a very pale yellowiſh brown, or wine colour: Its taſte was alkaline, mixt with that of bitter almonds. Two drachms of water mixt with 20 drops of a concentrated ſolution of muriate of iron, when decompoſed by this pruſſiated alkali, afforded four grains of pruſſian blue, ſtript from the adhering oxide of iron by muriatic acid.

Product very little.

2 oz. blood, 1 carb. alk. low heat. Product not increaſed.

Two ounces of dried blood and one of carbonate of potaſh, treated in a ſimilar manner, yielded, by being mingled with a like quantity of muriate of iron, 4½ grains of pure pruſſian blue.

(The Remainder in our next.)

Obſervations

XVI.

Obſervations on the Cauſe which augments the Intenſity of the Sound in Speaking Trumpets. By J. H. HASSENFRATZ *.

Antiquity of th ſpeaking trumpet.

ALTHOUGH the ſpeaking-trumpet is an inſtrument which has been long known, ſince Kircher is of opinion that Alexander uſed it to command his army, and Solard made one in Paris, in 1654, from the deſcription which Kircher had given of that of Alexander, it was not, however, until 1671, when Morland made known that which he had conſtructed, and invited men of ſcience to determine the figure moſt proper for this inſtrument, that the ſpeaking-trumpet was really known among us, and began to be uſed.

Morland's.

It appears that Morland did not adopt the conical form terminated by a mouth piece, which he gave to the ſpeaking trumpets of glaſs, iron and copper, he made until after a ſucceſſion of trials on the beſt form to be given to them, to make them produce articulate ſounds.

Caſſegrain's.

It alſo appears that it was without knowing it, and by chance, that Caſſegrain gave the ſpeaking trumpet, which he made in 1672, the hyperboloidal form firſt noticed by Sturm.

Were at firſt conſtructed without known principles.

Until 1719, when Haſſe publiſhed a diſſertation on the improvement of ſpeaking trumpets, this inſtrument was conſtructed without principles; for we neither can nor ought to conſider as principle, the harmonic proportion mentioned by Caſſegrain as being neceſſary between the length and the width of the tubes of ſpeaking trumpets.

Haſſe's theory of the augmentation of the intenſity of ſound in them.

The law of the reflection of ſound in echoes, led Haſſe to apply the theory of catoptrics to ſpeaking trumpets, and induced him to conſider the combination of the *ellipſoidal* and *paraboloidal* forms as the moſt advantageous for this inſtrument: but this union not producing the effect which the profeſſor of Wirtemberg had hoped for, it was abandoned, and in conformity with his opinion, the augmentation in the intenſity of the ſound continued to be conſidered as the product of the reflection of the ſonorous rays in the ſpeaking trumpet, and of the vibration of the ſubſtance of which it was compoſed. It was for this reaſon that, in the fabrication of theſe inſtruments, the attention was directed to having a hard, elaſtic and thin ſubſtance, that it might vibrate in uniſon with all the tones, and reflect the ſonorous rays which ſtruck it in all directions.

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* From Annales de Chimie, Praireal, An. XII.

Lambert's theory. Vibration injures the articulation of the ſounds.

In 1763, Lambert withdrew from the theory of ſpeaking trumpets, the vibration of the materials of which they are compoſed: he ſhowed that the vibration of the ſides which is calculated to augment the intenſity of a long continued ſound, would render articulate ſounds confuſed, which muſt ſucceed each other rapidly; that in this caſe therefore it would be neceſſary to ſpeak extremely ſlow, and that, even by ſpeaking ſlowly, it would be impoſſible to diſtinguiſh the conſonants, which are only momentary modifications of the vowels; that the latter, pronounced in the trumpet, would be ſo ſonorous, that the conſonants muſt be gueſſed at, which would be extremely difficult. By ſprinkling the exterior ſurface of a tin ſpeaking-trumpet with ſaw-duſt, I ſatisfied myſelf that the ſurface vibrated in ſome circumſtances, but I was equally ſatisfied, on covering the outſide of the trumpet with a ſoft looſe ſtuff to ſtop and obſtruct its vibrations, that the intenſity of the ſound was no leſs ſtrong, in this ſecond caſe, and, that thus the vibration of the ſubſtance of the trumpet was, at leaſt, uſeleſs, if it was not injurious to the diſtinctneſs of articulated ſounds.

Speaking trumpets do vibrate, but the ſound is equally ſtrong when the vibration is prevented.

In his memoir, publiſhed among thoſe of the Academy of Berlin, for the year 1763, Lambert has attributed all the augmentation of the intenſity which the ſound experiences by ſpeaking into a trumpet, to the reflection of the ſonorous rays on the ſmooth and poliſhed ſurface of the interior of this inſtrument; he ſays that, of all the forms, that beſt calculated to concentrate the ſound by reflecting it, and the moſt eaſy to conſtruct, is the conic form: he afterwards ſhows that the ſound is ſtrengthened in conical ſpeaking trumpets, in the proportion of double the length of the cone which forms the trumpet to double the ſize of half the angle at the ſummit of the cone, ſuppoſing the length of the cone to be equal to the radius. If the angle of the cone is made $= \varphi$, the ſound is ſtrengthened in the proportion $\sqrt{2} : 2\ (\text{ſin.}\ \tfrac{1}{2}\varphi)$.

Proportions of a conical ſpeaking trumpet.

Seeking, from theſe formulæ, to diſcover what would be the beſt proportions to give to conical ſpeaking trumpets, he found that whatever the angle of the ſummit of the cone was, its baſe muſt be equal to the diſtance between the ſummit of the cone and the mouth of it, and that its length muſt be equal to the diameter of the mouth, divided by four times the ſquare of the ſine of half the angle of the cone: that thus in a trumpet of ſix feet in length, the angle of the cone muſt be 16° 17, and that ſuch a ſpeaking-trumpet would make the ſound 96 times more intenſe.

Lambert therefore, as well as thoſe who have preceded him, conſidered the augmentation of the intenſity of the ſound in ſpeaking trumpets, as the reſult of the action of the reflection of the ſonorous rays againſt the interior ſides of the inſtrument.

Theſe inſtruments have latterly been conſtructed without attending to the cauſes of the increaſe of ſound.

It would appear that ſince Lambert's memoir, ſpeaking trumpets have been made uſe of, without attending to the cauſes which augment the intenſity of the ſound, and, in fact, it is ſeen that in all works in which this inſtrument is ſpoken of, its effects are attributed either to reflection alone, or to reflection combined with the vibration of the ſubſtance of which it is formed.

Theory of ſpeaking trumpets and other inſtruments.

If we compare the theory of ſpeaking trumpets with that of the inſtruments which have ſome reſemblance to it, ſuch as trumpets and hunting horns, we muſt be aſtoniſhed to find that they are referred to different principles. In the ſpeaking trumpet, the cauſe is attributed to the reflection of the ſound; in the other inſtruments to the vibration of the air contained in the tube. Why theſe two cauſes, while the effects are analogous? This analogy has led me to inquire if, in reality, the reflection produced the augmentation of the intenſity of articulation in horns and acouſtic tubes, as well as in ſpeaking-trumpets, as is generally believed.

Does reflection increaſe the ſtrength of ſound?

Animals do not hear by the reflection of ſound.

On examining the ears of animals, it is ſeen that the greateſt number have an exterior auricle, which moſt of them direct towards the place from which the ſound proceeds; this auricle being, in many animals, covered inwardly with hair, which ſtops and hinders the reflection of the ſound, it is reaſonable to conclude that it is not by reflection that the ſound is tranſmitted into the ear.

Ear-trumpets do not tranſmit ſound by reflection.

When afterwards we obſerve the form given to acouſtic horns, which is that of a cone, the ſummit of which, ſlightly truncated, is placed in the ear, we are quickly led to concludo that the ſound is not tranſmitted by reflection; for the angle of the incident ray being increaſed at each reflection by that of the cone, after a number of determinate reflections, the angle would become larger than a right angle, and the ray would return upon itſelf. Thus the greateſt number of the rays would iſſue out of the mouth, after being ſeveral times reflected, and the quantity of the rays which thus reach the ear, and which are proportionalby leſs numerous as the cone is longer, cannot ſenſibly augment the intenſity of the ſound. Nevertheleſs, acouſtic horns do ſtrengthen the ſound conſiderably; this augmentation muſt therefore depend on another cauſe.

Horns of a paraboloid form diſuſed.

It has been propoſed to make horns of a paraboloidal form, as being beſt calculated to tranſmit ſound by reflection; but whether they did not produce the effect which was expected from them, or that they were too difficult to conſtruct, theſe horns have been abandoned, and none but conic horns are made uſe of.

Cylindrical acouſtic tubes tranſmit ſound without reflection to a great diſtance.

The acouſtic tubes employed to tranſmit the ſound to great diſtances, ſo that it cannot be heard by perſons placed between the two extreme points, are uſually formed of cylinders, which ſerve to conduct the ſound. Lambert had already ſtated that reflection did not increaſe the intenſity of ſound in cylinders, becauſe whatever might be the direction of the incident rays, being conſtantly reflected between two parallels, the angle which they form with the axis of the cylinder muſt be the ſame at entering and quitting it; that thus the ſound muſt experience a diminution at its iſſue, which will be ſo much the greater as the number of reflections is more conſiderable. Nevertheleſs the ſound is tranſmitted to a great diſtance by means of theſe tubes, and I have aſcertained that the beating of my watch, which ceaſes to be audible at the diſtance of 1^m. 10, the medium of ſeven experiments *(A)*, is heard at the diſtance of 2^m 25, the mean diſtance of ſeven experiments *(A)* when I place it in the mouth of a paſteboard tube, 0^m. 038 in diameter and 0^m. 6 in length; whence it follows that this tranſmiſſion muſt proceed from another cauſe.

According to the theory of reflection the enlarged part (pavillion) is of no uſe.

All theſe conſiderations have therefore induced me to examine the effects of the ſpeaking trumpet with the greateſt care. It follows from the theory of the reflection of ſound, applied by Lambert to the ſpeaking trumpet, that the opening by which it is uſually terminated, ſhould be at leaſt uſeleſs, if not injurious, for it has no influence on the concentration of the reflected rays; and therefore this gentleman propoſed to lay it aſide entirely. The analogy of form between the trumpet and the ſpeaking trumpet not being a ſufficient reaſon for retaining it, it is probable that it would have been laid aſide, in the ſame manner as the contour of the trumpet was abandoned, when the form and dimenſions moſt ſuitable to this inſtrument were ſought for by experimental trials. There would alſo have been another inducement to diſpenſe with it, becauſe Haſſe had already omitted it in the ſpeaking trumpet which he indicated as preferable to thoſe of Morland and Caſſegrain. I aſcertained by experiment that two ſpeaking trumpets of the ſame length and the ſame diameter produced different effects when they were terminated by an enlarged aperture or not, and that in general this enlargement conſiderably increaſed the intenſity of the ſound. The beating of my watch which I heard at 4^{m}. 2, the mean diſtance of ſeven experiments *(a)* by placing it at the mouth of a ſpeaking trumpet of 0^{m}. 6 long and 0^{m}. 38, mean diameter of the tube, was not heard beyond 2^{m}. 25, mean diſtance of ſeven experiments *(a)* when I placed it in a tube of the ſame length and the ſame diameter. Thus the cauſe which occaſions this aperture to ſtrengthen the ſooner is different from the reflection of the ſonorous rays.

Which is contrary to experiment.

Since the reflection of ſound does not in any manner concentrate the ſonorous rays in a cylindrical tube, it muſt follow, as Lambert has concluded in his Theory of Reflection, that ſpeaking trumpets with cylindrical tubes ſhould not ſenſibly augment the ſound. To prove whether experiment agreed with this theory, I immediately conſtructed a ſpeaking trumpet, with a cylindrical tube of 0^{m}. 36, the length of the cylinder, 0^{m}. 25, the length of the enlarged part, 0^{m}. 035, the diameter of the cylinder, and 0^{m}. 190, the greateſt diameter of the enlarged part, and, by comparing the intenſity of the ſound which it produced, with that of a conical ſpeaking trumpet of the ſame height, and of a ſimilar mean diameter, I aſcertained that the ſtrength of the ſound was ſenſibly the ſame. In both, the beating of my watch, which I did not hear in the open air beyond 1^{m}. 08, mean diſtance of five experiments *(b)* was heard at 3^{m}. 94, mean diſtance of five experiments *(b)* when I placed it at the mouth of either ſpeaking trumpet. Since the cylindrical ſpeaking trumpet ſtrengthens the ſound in the ſame manner as that which is conical, and ſince, on the contrary, the theory of reflection ſhows that it ſhould not be ſtrengthened, it follows that the augmentation of ſound in theſe inſtruments ariſes from a cauſe different from reflection.

This is further proved by the ſound being augmented in ſpeaking trumpets with cylindrical tubes, as ſtrongly as in thoſe of a conical form.

One

The intenſity of the ſound is not leſſened by deſtroying the re-reflection by means of an internal woollen covering.

One deciſive experiment remained to be made with theſe inſtruments, this was to deſtroy and to reſtore the action of the reflection in a ſpeaking trumpet, ſuppoſing it to take place, and to compare the intenſity of the ſound obtained in theſe two circumſtances, in order to determine the proportion of the effect produced by this reflection. I conſequently procured a ſpeaking trumpet of woollen ſtuff, which I placed within one of tin, and I obſerved that the intenſity of the ſound obtained with or without the ſtuff in the inſide, was ſenſibly the ſame; whence it follows that the reflection has no appreciable influence on the augmentation of ſound in a ſpeaking trumpet.

Except inaſmuch as it diminiſhes the diameter of the inſtrument.

I have ſaid that the intenſity of the ſound was ſenſibly the ſame; nevertheleſs I muſt obſerve that it was diminiſhed; for the beating of my watch, placed in the mouth of the bare ſpeaking trumpet was heard at a diſtance of 3^{m}. 94, mean of five experiments *(b)*, while when placed in the mouth of the ſame inſtrument covered in the inſide with the ſtuff, it was not heard beyond 2^{m}. 48. This difference, which at the moment might be ſuppoſed to ariſe from the reflection of the ſound taking place in the one caſe, and not in the other, ſeems to depend ſolely on the diminution of the diameter of the cylinder of the ſpeaking trumpet and of the enlarged parts occaſioned by the ſtuff placed within them: I ſatisfied myſelf that the ſpeaking-trumpet which I made uſe of *(b)*, preſerving all its dimenſions and experiencing no difference except in the diameter of the tube, produced a diminution in the intenſity of the ſound, whenever a ſmaller diameter was made uſe of; thus, in the ſpeaking trumpet already mentioned *(b)*, the beating of my watch was heard at a diſtance of 3^{m}. 94, when the diameter of the tube was 0^{m}. 035; it was not heard beyond 3^{m}. 08, when the tube was only 0^{m}. 032; finally, it was not heard beyond 2^{m}. 34, when the tube was 0^{m}. 028. It is evident, therefore from theſe experiments, that the diminution in the intenſity of the ſound by covering the interior of a ſpeaking trumpet with ſtuffs, ariſes from the diminution of the diameter of its tube.

The diminution in the intenſity of the ſound is compenſated by its clearneſs.

I ſhould obſerve that if the internal ſtuff occaſions an inconſiderable diminution in the intenſity of the ſound, it, on the other hand, produces a great advantage by rendering the articulated ſounds clearer and leſs confuſed.

General concluſions.

It follows from all which has been ſhewn on the auricles of animals, on horns, and acouſtic tubes, and on ſpeaking trumpets,

pets, that the augmentation of ſound, in every circumſtance, is not owing to the reflection of ſonorous rays, conſequently there exiſts no reaſon for ſeparating the cauſe which augments the ſound in theſe inſtruments, from that which ſtrengthens it in trumpets and hunting horns; that the different ſounds produced in the two caſes are owing to the vibration of the air in the tubes, and their ſtrengths or their intenſities, to the augmentation of the amplitude of their vibration, ariſing from the greater impulſe which the air neceſſarily receives when it is encloſed in a tube. General concluſions.

Experiment (A).

The tubes and the ſpeaking trumpet of paſteboard.

The interior diameter of the tube of the ſpeaking trumpet is $0^m.038$, that of the exterior diameter of the enlarged part $= 0^m.210$. The length of the tube $= 0^m.4$, and the length of the enlarged part $= 0^m.2$.

	With a tube of the length of			The ſpeaking trumpet.	Without tube or ſpeaking trumpet.	Obſervations.
	m. 0.4	m. 0.6	m. 0.10			
Diſtance at which the beating of the watch was heard.	1.33	1.66	2.33	4.00	1.00	The great difference between theſe experiments ariſes, 1ſt, from the diſpoſition of the organ;—2d, from the greater or leſs noiſe produced in the neighbourhood of the place where the experiments were made.
	2.24	2.90	3.33	4.66	1.16	
	2.16	2.66	3.33	5.00	1.33	
	1.33	1.79	1.85	3.00	0.83	
	1.92	1.95	2.00	5.00	1.33	
	1.58	2.16	2.66	3.66	1.00	
	2.54	2.66	3.17	4.00	1.00	
	12.86	15.78	18.67	29.33	7.65	
Mean	1.83	2.25	2.67	4.20	1.10	

Experiment (B).

The ſpeaking trumpet was of tin, as well as the tubes.

The interior diameter of the tube of the ſpeaking trumpet $= 0^m.035$, that of the exterior diameter of the enlarged part $= 0^m.290$, the length of the tube $= 0^m.36$, and that of the enlarged part $= 0.25$.

	With an internal diameter of			Without ſpeaking trumpet or tube.
	m. 0.035	m. 0.032	m. 0.028	
Diſtance at which the beating of the watch was heard.				m.
	4.88	3.58	2.76	1.48
	3.58	2.93	2.28	0.97
	3.25	2.76	2.28	1.30
	4.39	3.58	2.44	0.82
	3.58	2.58	1.95	0.82
Sum	19.78	15.43	11.71	5.39
Mean	3.94	3.08	2.34	1.08

XVII.

Account of Cerium, a new Metal found in a mineral Subſtance from Baſtnas, in Sweden. *By* W. D'HESINGER *and* J. B. BERGELIUS *.

§ I.

Deſcription of the Tungſtein of Baſtnas.

Diſcovery of a new ſubſtance in tungſtein.

ALTHOUGH this ſubſtance has been formerly aſſayed by Scheele and D'Elhuyar, under the name of wolfram, its conſiderable weight, nevertheleſs, determined us to ſubmit it to farther enquiries. Our principal object was to find yttria in it, which, being unknown at the time in which theſe chemiſts operated, might have eſcaped their attention. Our ſuppoſition was not well founded, ſince, inſtead of an earth, we diſcovered a ſubſtance which, according to every appearance, is hitherto unknown, as will be ſeen in the ſequel.

Where found.

The tungſtein of Baſtnas, which we call *cerite*, for reaſons which will be preſently given, was found, in the year 1750, in a copper-mine called Baſtnas, or Saint-Gorans Koppargrufva, at Riddare-Hyltan, in Weſtmannia, of which, with aſbeſtos, it formed the matrix: but after this time it was imbedded in quartz and mica, to the depth of ſeventeen toiſes.

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* From the Swediſh, by G. A. Linborn; but here tranſlated from the Annales de Chimie, L. 145.

The

Deſcription the ore.

The tungſtein is almoſt always mechanically mixed with black amphibole *(hornblende)*, ſtriated actinote, of a clear green colour *(ſhorl)*, mica, ſulphurated copper, biſmuth, and ſulphurated molybdena, which may be eaſily known by expoſing it to the fire.

Phyſical pr ties of cerit

The cerite, properly ſo called, is tranſparent, of a fleſh colour, ſometimes deep and ſometimes bright, ſeldom yellowiſh. In a maſs, or in ſmall ſpecimens, the ſtone is of an irregular form; its fracture is indeterminate, compact, and a little brilliant, with obtuſe edges; its conſiſtence is tenacious and ſtrong; it gives fire with ſteel with difficulty, but does not ſcratch glaſs; it is not attracted by the magnet; after having been made red-hot in the fire, it loſes its hardneſs, and ſix or ſeven *per cent.* of its weight: by this operation it becomes friable, and acquires a bright yellow colour: it does not melt alone.

Is the falſe ſtein of Sc

On account of its weight, Cronſtedt has placed it among the tungſteins, in his Mineralogy. In pure fragments, it is to that of water as 4.733 and 4.935 to 1.000. Scheele not having found wolfram in it, called it falſe tungſtein.

Conſtituent principles.

The conſtituent principles of this mineral were given by Bergman, in the Memoirs of the Academy for the year 1784, page 121, from an analyſis of D'Elhuyar: they are as follows:—

Silex, - - -	0.22
Iron, - - -	0.24
Lime, - - -	0.54
	1.00

Habitude w borax.

Heated with borax by the blow-pipe, it forms a globule of glaſs, which, while hot, appears greeniſh, but is colourleſs when cold. Urged with carbonate of ſoda in a platina ſpoon it is not diſſolved.

§ II.

Analyſis of the Proportions of Cerium.

Treatment the ore wit tric acid,

TO ſeparate the yttria which was ſuppoſed to be in it, it was reduced to a fine powder in a porphyry mortar; pure concentrated nitric acid was then poured upon it. The acid was decompoſed, and a conſiderable quantity of nitrous gas and carbonic acid gas were diſengaged. The ſtony powder was repeatedly treated with this acid, until the inſoluble reſidue appeared white.

The ſolution diluted with water was of a yellow colour, which became greeniſh by boiling, and afterwards red: completely dried, it became of a yellowiſh white, but regained its red colour by attracting humidity. It is entirely diſſolved in alcohol; and the ſolution, ſlightly digeſted, depoſits a conſiderable quantity of oxide of iron. It likewiſe depoſits more oxide of iron by remaining for ſome days undiſturbed in a window. The decanted ſolution, being almoſt clear, was evaporated to ſiccity, and the calcined ſalt was in the form of a powder, of the colour of bricks. Water could only diſſolve the calcareous earth. Diſtilled vinegar could only take up a very ſmall portion, and was not ſaturated, though aſſiſted by the heat of ebullition. The evaporated acetic ſolution gave ſmall granulated cryſtals, of a ſaccharine aſtringent taſte. They were not totally ſoluble in alcohol. The part of the acetous ſalt which was not diſſolved in alcohol, gave, by calcination, a brick-coloured powder, reſembling that which had not been diſſolved.

Ammonia precipitated the alcoholic ſolution in a white powder, which became yellowiſh in the air. It was a little ſoluble by carbonate of ammonia, and acquired the colour of bricks by calcination. The ſediment being ſeparated, the carbonate of ammonia produced a white precipitate, which was pure carbonate of lime. The acetous ſalt did not therefore contain yttria. The powder from which the calcareous earth had been ſeparated, diſſolved in muriatic acid, with a diſengagement of oxigenated muriatic acid gas, which indicated that there was a metallic oxide.

Indications of a metallic ſubſtance.

Was it oxide of mangaueſe combined with oxide of iron?

To aſcertain this, we endeavoured to develope the pure oxide of manganeſe by means of tartrite of potaſh, according to Richter's method. We decompoſed in this manner, a ſolution of this ſubſtance in muriatic acid, perfectly neutralized by tartrite of potaſh; and after having waſhed the precipitate well, we ſubmitted it to a ſlow calcination; but it only produced the brick-coloured powder.

Does not contain alumine.

Cauſtic alkali had no action on the inſoluble part of the nitrate; which proves that it did not contain alumine.

To obtain the pure metallic matter in a ſufficient quantity to make ſeveral aſſays, another portion of cerite was diſſolved in nitric acid, and the ſolution evaporated to ſiccity. Water was poured on the reſidue, and it was precipitated with ammonia. The waſhed precipitate was diſſolved in nitric acid. The ſolution, well neutralized with the alkali, was afterwards precipitated by tartrite of potaſh. A white powder was alſo precipitated from the ſame ſolution by carbonate of potaſh, but it was in ſmall quantity. Theſe precipitates were ſeparately calcined, and both of them acquired the colour of bricks. The precipitate formed by the carbonate of potaſh, was not diſſolved by potaſh aided by digeſtion; it therefore did not contain alumine. The iron contained in the ſolution, precipitated with tartrite of potaſh, was ſeparated by hidro-ſulphuret of ammonia. The remainder of the ſolution of cerite in nitric acid, which had been precipitated by cauſtic ammonia, gave carbonate of lime by carbonate of ammonia.

Component parts of the ore 50 per cent. of oxide of cerium

From theſe aſſays it reſults, that cerite contains nearly 23 parts of ſilex, 5.5 of carbonate of lime, 22 of oxide of iron, and a quantity of this metallic matter, the weight of which, after calcination, rather exceeded 50 *per cent.* But this ſubſtance being then, as well as the iron, united with more oxigen than they contained in the cerite, we have, inſtead of a loſs, an augmentation of weight, which probably ariſes from the oxigen. Neither is the loſs which the cerite experienced in the calcination, included in this account. We alſo found traces of manganeſe, but in ſo ſmall a quantity, that potaſh, melted with the cerite and diſſolved in water, did not give any colour.

Not having the practice which complete proportionate analyſes require, we offer theſe reſults with diffidence, and in the hope that ſcientific men of more experience will employ themſelves on this ſubject.

§ III.

Examination of the Metallic Oxide found in the Cerite.

Examination of the oxide.

A PASTE was made of 37 grains of this oxide and linſeed oil, which was reduced in charcoal in a covered crucible. Although it retained ſome carbon, it loſt half a grain of its weight. This maſs was incloſed in a lined crucible without any flux, and M. Hjelm expoſed it for half an hour to the degree of fire neceſſary for the reduction of manganeſe. The oxide was not melted, but reduced into a very fine powder: it exhibited brilliant particles in the day-light, and ſtained white paper black. It diſſolved in muriatic acid, diſengaging, at the firſt, ſulphurated hidrogen gas, and afterwards pure hidrogen gas. This colourleſs ſolution had a ſaccharine taſte. Thus it appeared to us that the metal was reduced in part. The origin of the ſulphur may be traced to the ſulphuric acid, from which the matter had been ſeparated by the cauſtic ammonia. The influence which this acid exerciſes in theſe aſſays, will be ſeen by the ſubſequent enquiries.

M. Gahn, at Fahlun, having more convenient furnaces, has promiſed to undertake the reduction of this ſubſtance with more power: if this operation ſucceed, we ſhall give an account of it hereafter.

It is the oxide of a metal not yet known.

Theſe appearances, and thoſe which follow, determined us to conſider the ſubſtance found in the cerite, as the oxide of a metal hitherto unknown, to which we have given the name of Cerium, from the planet Ceres, diſcovered by Piazzi.

Manner of obtaining the pure Oxide of Cerium.

Proceſſes for obtaining the pure oxide.

(A.) Pure uncalcined cerite was diſſolved in nitro-muriatic, and, after ſaturating the clear ſolution with the alkali, was precipitated by tartrite of potaſh. The precipitate well waſhed, calcined, and digeſted in vinegar, contains the pure oxide of cerium.

Or otherwiſe decompoſe a ſolution of cerium in nitro-muriatic acid, ſtill warm, but not ſaturated, by ſuccinate of ammonia: a ſuccinate of iron is gradually depoſited. The precipitation is to be continued by means of ſuccinate of ammonia, as long as a red precipitate is formed: the ſolution is then nearly deprived of iron. After having ſeparated the ſuccinate of iron, more ſuccinate of ammonia is poured into it, until a white precipitate appears. The ſolution is then left at reſt, in order that the ſmall portion of ſuccinate of cerium may be depoſited. The iron diſſolved by the free muriatic acid, is depoſited at the ſame time, and the ſolution is freed from this metal. The cerium may afterwards be precipitated by ammonia, and then waſhed and calcined.

Of the Properties of Oxide of Cerium.

The oxide is capable of di ferent degree oxidation.

(B.) This oxide may appear in different degrees of oxidation. The alkalies precipitate a white oxide from its ſolutions, which ſhows of a yellowiſh colour in the air, but, when perfectly dried, becomes dark. Expoſed to a briſk and long-continued fire, it takes a deep brick-colour. The oxalate and acetate of cerium, calcined in veſſels not completely cloſed, yield a white oxide, which, in an open fire, becomes of the colour of bricks. It does not melt by itſelf.

Fuſion with fluxes.

Treated with borax by the blow-pipe, it melts readily and ſwells. The globule heated by the exterior flame, aſſumes the colour of blood; which, by cooling, paſſes to a yellowiſh green, and at length becomes colourleſs, and perfectly tranſparent. Melted by the interior flame, theſe changes do not take place; it is then reduced into a colourleſs glaſs; but, expoſed for a ſhort time in the exterior flame, the ſame phenomena are produced. If too much oxide of cerium is made uſe of, the glaſs reſembles an opake yellowiſh enamel. Theſe changes are more eaſily manifeſted with the phoſphate of ſoda and ammonia. If two clear and colourleſs globules are melted together, one of which was prepared with borax and the other with the phoſphate, they form a tranſparent glaſs, which, on cooling, becomes opake and pearl coloured.

Theſe characters, taken together, ſufficiently diſtinguiſh the oxide of cerium from the oxide of iron. The latter alſo offers the ſame changes of colour, but its glaſs, on cooling, has a deep green colour, *which fades*. The globules made with borax and the phoſphate melted together, yield an opake glaſs, the colour of which is a little deeper.

Oxide of Cerium treated with Sulphuric Acid.

Sulphate of rium at a m mum of oxid tion.

(C.) When oxide of cerium is digeſted with ſulphuric acid, theſe two ſubſtances unite, and the reſult is a red inſoluble ſalt, which is ſulphate of cerium at a *maximum of oxidation.* If the acid is concentrated, it ſcarcely diſſolves any of it. If it is diluted with half its quantity of water, or a little more, the reſult is a yellowiſh oily liquor, which does not adhere to the glaſs, nor does it wet it. If the acid is mixed with ſix or ſeven times its quantity, or even more, of water, and employed in a ſufficient quantity, the oxide is diſſolved of an orange

Acidulous ſulphate of cerium.

Neutral acid.

Acidulous ſulphate of cerium but ſlightly oxigenated.

orange colour. By a ſlight evaporation of this ſolution, it yields ſmall, priſmatic, coherent cryſtals, of the colour of gold. This ſalt is an acidulous ſulphate of cerium at a *maximum*. Theſe cryſtals, thoroughly dried between blotting-paper, and expoſed to the air, are gradually reduced to a yellow, almoſt cryſtalline powder. Rediſſolved in water, they experienced a decompoſition; a white powder is depoſited, and the ſolution becomes colourleſs. This white powder is a ſulphate of cerium, but little oxigenated. If the ſolution is evaporated to dryneſs, it gives an acidulous ſulphate of cerium ſtill leſs oxigenated. Theſe cryſtals are ſeldom cubical, but almoſt always priſmatic, ſtriated and collected in bundles. Their taſte is ſour, but they afterwards become ſaccharine and aſtringent.

Muriatic acid deprives the acidulous ſulphate of part of its oxigen:

As does an increaſe of temperature, which afterwards drives off the acid, and forms a neutral ſulphate

Calcination reſtores the oxigen.

Treated with muriatic acid, the yellow acidulous ſulphate of cerium yields part of its oxigen to the acid, which is volatilized in oxigenated muriatic acid gas. The ſalt remains colourleſs. An augmentation of temperature alone is ſufficient for the yellow acidulous ſulphate of cerium to loſe its colour by loſing the exceſs of its oxigen. If the heat is increaſed ſtill more, the ſurplus of the acid is carried off, and a ſaturated ſulphate of cerium remains. By a continued calcination, it regains oxigen, becomes red, and yields a ſulphate of cerium at a *maximum*. The ſulphate of cerium, diſoxigenated by the muriatic acid, is more difficult to re-oxidate by calcination.

Action of the alkalies on the ſulphates of cerium.

In the humid way, the alkalies only decompoſe the ſulphate of cerium incompletely. Ammonia precipitates an oxide from the acidulous ſulphate of cerium, which is only in a ſmall quantity, but nevertheleſs contains part of the ſulphuric acid. The ſulphate of cerium is not perfectly decompoſed, except by calcination with three times its weight of carbonate of ſoda or potaſh. The calcined oxide is of a brown colour. By digeſtion, ammonia can deprive it of part of its acid: the oxide takes a diſtinct fleſh colour, which becomes brighter by drying. Digeſted with concentrated muriatic acid, or with nitric acid, a ſmall quantity diſſolves, having its yellow colour.

Sulphate of cerium and potaſh.

If a ſolution of acidulous ſulphate of cerium is precipitated by potaſh, a triple combination of cerium, ſulphuric acid, and potaſh, is ſeparated, before the acid is ſaturated. If too much potaſh is added, the combination is partly deſtroyed. The ſulphate of cerium and potaſh, at a *maximum*, is of an orange colour; that which is at a *minimum*, is white. A ſimilar combination is alſo obtained by pouring muriate of cerium into a ſolution of ſulphate of potaſh. Sulphate of ammonia does not form any precipitate in it; but, on adding a calcareous ſalt to it, ſulphate of cerium combined with potaſh is rapidly depoſited.

Sulphuric acid in exceſs frees ſulphate of cerium from iron.

Theſe characters offer a ready method of ſeparating the ſulphate of cerium from iron. It muſt however be obſerved, that when the ſolutions are ſaturated, a little iron is alſo depoſited, which gives a yellow colour to the precipitate; but by adding a little ſulphuric acid in exceſs, the iron is rediſſolved, and leaves the precipitate entirely white. This combination is only diſſolved in part by dilute ſulphuric acid, and the greateſt quantity of that ſeparates afterwards.

Sulphate, and carbonate of cerium.

The ſulphate of cerium and potaſh melts by a ſtrong heat. Heated with charcoal, it gives ſulphuret of potaſh and ſulphuret of cerium. Melted with carbonate of potaſh, in cloſed veſſels, it yields carbonate of cerium and ſulphate of potaſh. This ſalt contains only one-third of oxide of cerium.

Concentrated nitric acid decompoſes ſulphate of cerium and potaſh.

It is diſſolved in concentrated nitric acid, and, during the cooling, an acid ſalt, formed of acidulous ſulphate of potaſh and a little ſulphate of cerium, cryſtallizes. Thus the ſulphate of cerium combined with potaſh, is decompoſed by concentrated nitric acid. This acid carries off the metal, and the ſulphuric acid is directed wholly upon the potaſh, with which it forms a ſalt with exceſs of acid.

Oxide of Cerium with Nitric Acid.

Nitrate of cerium.

(D.) Nitric acid diſſolves the calcined oxide with difficulty, but that which is precipitated by pure or carbonated alkalies, with eaſe. When the ſolution is ſaturated with oxigen, it is of a greeniſh yellow colour; but colourleſs, when leſs oxided. Evaporated to the conſiſtence of honey, it depoſits lamellated cryſtals, which attract the humidity of the air. The ſolution has a saccharine taſte: like all the other ſaturated ſolutions of cerium, it lets fall an oxide of cerium, at a *maximum* of oxidation, in the open air. This precipitate is frequently formed of oxide of iron. When dry, this ſalt is of a yellowiſh white colour; but becomes colourleſs on being diſſolved in a ſufficient quantity of water. It diſſolves readily in alcohol.

A concentrated ſolution of this ſalt takes a blood colour, on account of a ſmall quantity of iron, which, by drying, paſſes to yellowiſh white, but is reſtored by a new ſolution.

A colourleſs and leſs oxided nitrate of cerium, is obtained by diſſolving the yellowiſh ſalt in alcohol: the ſolution inflames and yields a white ſalt.

It is deſtroyed by fire, which drives off its acid.

With Muriatic Acid.

Muriate of cerium.

(E.) The calcined oxide of cerium is ſlowly diſſolved in muriatic acid in the cold, and more readily by heat; an efferveſcence is produced, owing to a diſengagement of oxigenated muriatic acid gas. The taſte of the ſolution is ſaccharine and aſtringent; the colour is a very faint greeniſh yellow: the dried ſaline maſs is yellowiſh white, and attracts humidity. We only ſucceeded once in obtaining it cryſtallized. The cryſtals were white, brilliant, in four ſided priſms, with the points cut off. The ſalt diſſolves readily in alcohol, and its concentrated ſolution burns with a yellow and ſparkling flame. The reſidue of the ſalt is white and gives a colourleſs ſolution. It is muriate of cerium at a *minimum* of oxidation.

Heated in cloſed veſſels, the water of cryſtallization is firſt diſſipated, afterwards the acid paſſes in the form of oxigenated muriatic acid gas. If the operation is ſtopped before the acid is entirely volatilized, an undecompoſed muriate of cerium, at a *minimum* of oxidation remains.

If the muriate of cerium contains iron, it all ſublimes in a brown deliqueſcent maſs. Nothing remains in the matraſs but a white oxide of cerium, which attracts the humidity of the air, and becomes yellow. Thus, ſublimation with muriate of ammonia may be employed to purify a muriate of cerium which contains a little iron.

With Phoſphoric Acid.

Phoſphate of cerium.

(F.) Free phoſphoric acid, ſaturated with an alkali, precipitates muriate of cerium. The precipitate is white, and ſoluble in muriatic acid and in nitric acid employed in ſufficient quantity.

This ſalt is alſo obtained by digeſting pure oxide of cerium, moiſtened with phoſphoric acid. It is not ſoluble in an exceſs of this acid.

With

With Carbonic Acid.

Carbonate of cerium.

(G.) The carbonate of ammonia precipitates muriate of cerium without efferveſcence. After the precipitation, carbonic acid is ſlowly diſengaged in the form of bubbles. The reſidue retains its acid, even after deſiccation.

Dry carbonate of cerium has a white colour tending a little to bluiſh or greeniſh. It diſſolves in the acids with efferveſcence. It does not loſe its acid in an open fire. In cloſed veſſels, without the contact of oxigen, it ſupports a gentle calcination, without being decompoſed.

With Arſenic Acid.

Arſeniate and acidulous arſeniate of cerium.

(H.) Free arſenic acid does not produce any change on muriate of cerium. The oxide digeſted with this acid, forms an inſoluble ſalt. An exceſs of this acid diſſolves it, and gives an acidulous arſeniate of cerium. The ſaturated arſeniate of cerium is depoſited in the form of a powder during the evaporation. The reſidue does not cryſtallize, but by deſiccation, becomes a gelatinous, clear, and colourleſs maſs.

With Molibdic Acid.

Molybdate of cerium.

(I.) The acidulous ſalts of cerium are not decompoſed by molybdate of ammoniac. The molybdate of cerium is precipitated from its ſaturated ſolutions, in the form of a white ſalt, which is not ſoluble in the acids.

With Oxalic Acid.

Oxalate of cerium.

(K.) Either the acidulous or ſaturated ſolutions of cerium are precipitated by oxalic acid. According to the degree of oxidation of the metal, the precipitate becomes red or white. This combination is alſo obtained by digeſting the oxide with free oxalic acid. An exceſs of acid does not diſſolve it, but ammonia readily effects its ſolution, giving it a yellow colour.

A ſmall quantity of oxide is depoſited by evaporation. The ſolution afterwards yields regular cryſtals in the form of needles. Pure alkalies do not occaſion any precipitate.

With Tartareous Acid.

Tartrite of cerium.

(L.) Free tartareous acid has no action on muriate of cerium. The recently precipitated oxide unites with this acid by digeſtion, and yields a tartrite of cerium, which diſſolves readily in water. This ſalt is alſo precipitated from ſaturated ſolutions by tartrite of potaſh. Like the oxilate of cerium, it diſſolves in pure ammonia, but does not cryſtallize.

Tartrite of cerium is not entirely ſoluble in water; the ſolution is precipitated by carbonate of ſoda.

With Benzoic Acid.

Benzoate of cerium.

(M.) This acid does not act on the muriate of cerium; but, by digeſtion, well concentrated benzoic acid diſſolves the oxide of cerium recently precipitated. On cooling, the ſolution firſt depoſits cryſtals of the acid in exceſs, and afterwards benzoate of cerium in the form of a white powder, which adheres to the cryſtals of benzoic acid.

The reſinous matter with which this acid is frequently united, combines with the benzoate of cerium, and forms an inſoluble brown powder.

With Citric Acid.

Citrate and acidulous citrate of cerium.

(N.) Muriate of cerium is not precipitated by citric acid. But the oxide digeſted with citric acid forms an inſoluble ſaturated combination, which an exceſs of acid diſſolves. The acidulous citrate of cerium is of a yellow colour and does not cryſtallize. Alcohol deprives it of its water, and of part of its acid, but does not diſſolve it.

With Acetic Acid.

Acetate of cerium.

(O.) The calcined oxide of cerium is only very imperfectly diſſolved in acetic acid, even with the aſſiſtance of heat; but that which is recently precipitated by the alkalies, is diſſolved with facility. The ſaturated acetate of cerium is ſoluble in water; it has a ſweet taſte, and gives granulated cryſtals, by evaporation, which do not change in the air, and are but ſlightly ſoluble in alcohol.

This ſalt ſwells in the fire, and is deſtroyed.

(To be continued.)

SCIENTIFIC

SCIENTIFIC NEWS.

Extract of a Letter from Mr. Bode, *Aſtronomer Royal at Berlin, to Mr.* A. F. Thoelden. *Dated the 23d of October,* 1804.

New planet Juno.

I HEREWITH tranſmit to you ſome of my own obſervations of the new diſcovered third planet, (which is to be called *Juno)* made at our Obſervatory.

		Mean Time.				Apparent R A.				Declin.		
Sept. 21	-	11h	55′	21″	-	359°	27′	46″	-	3°	38′	23″ South.
25	-	11	37	4	-	358	47	16	-	4	32	10
27	-	11	28	0	-	358	27	21	-	4	58	35
28	-	11	23	29	-	358	17	18	-	5	11	35
Oct. 10	-	10	28	42	-	356	29	13	-	7	36	56
14	-	10	11	9	-	356	1	14	-	8	18	51
20	-	9	45	22	-	355	29	24	-	9	12	46

Aſtronomical prize.

I alſo inform you, that an anonymous admirer of aſtronomy of Berlin, has depoſited with me the ſum of 20 Frederics d'or, for any important diſcovery, treatiſe, or ſatisfactory ſolution of any difficult problem, of, or relating to, aſtronomy, to be ſent to me between this and the end of Auguſt, 1805. (The concurrents are requeſted to write their diſſertations either in French, or in German, affixing a motto to each, with a ſealed note containing their name and addreſs). After the expiration of the above-mentioned period I am to conſult with my aſtronomical friends reſpecting the merits of the papers ſent, and to adjudge the premium accordingly, on condition, however, that I may be at liberty to make uſe of ſuch papers, addreſſed to me, as I think proper to inſert into my aſtronomical journals.

Signed, J. E. BODE.

N. B. Any aſtronomical gentleman who intends to become a competitor, may ſend his paper to me, and I will forward it to Mr. Bode without any expence.

A. F. THOELDEN.

No. 10, *St. Alban's Street, Pall-Mall,*
Nov. 3*d*, 1804.

Experiments

Experiment on the Heat which is developed during the Compreſſion of the Air[*].

Heat by compreſſion of air.

A VERY curious experiment has been lately repeated before the National Inſtitute. If the air be very rapidly compreſſed in the ball of an air gun, a conſiderable quantity of heat is diſengaged from the firſt ſtroke of the piſton, which is ſo great that it is capable of ſetting fire to a piece of fungus match (amadou) placed within the pump. If the body of the pump be terminated by a moveable end formed of a piece of ſteel firmly ſcrewed in, and furniſhed in its centre with a glaſs lens, which admits of the inſide being ſeen, at the firſt ſtroke of the piſton, a ray of vivid brilliant light will be perceived, which is ſuddenly diſengaged.

This obſervation is owing to accident; it was made for the firſt time by a workman in the manufacture of arms of Saint Etienne, who, on diſcharging an air gun in which the air was ſtrongly compreſſed, perceived a very ſenſible light at the end of the barrel.

Extract of a Letter from PROFESSOR PROUST *to* J. C. DELAMETHERIE[†].

AMONG the memoirs which I have had for a long while to ſend to you, you will find one on Berthollet's work, and on what he ſays of the hydrates, and alſo the analyſis of a ſtone fallen from the atmoſphere, at *Sigena*, in Arragon, which was found to be perfectly ſimilar to all thoſe which have been hitherto analyzed.

Brandy from the carobe tree.

I do not know whether I ſhould mention to you a ſort of diſcovery, which, however trifling it may appear to be, will, nevertheleſs be one day of great importance in Spain, particularly in the provinces where the carobe tree *(caroubier)* is cultivated. It is known that the fruit of this tree is only cultivated along all the coaſt of the Mediterranean, to feed cattle. However, after a ſuitable fermentation it yielded me a *quartillo* (about the bulk of a pound of water) of excellent brandy,

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[*] Bulletin des Sciences, III. p. 209. The latter obſervation of the flaſh from an air-gun has been ſeveral times obſerved before: See our Vol. IV. p. 280. It ſingularly contraſts the former fact. N.

[†] From Journal de Phyſique, &c. Meſſidor, An. XII.

Dutch proof, from five pounds of the dried fruit. This brandy, although it retains a ſlight odour of the fruit, is by no means diſagreeable to the taſte, and I have made liqueurs with it in no reſpect inferior to thoſe of commerce. I ſhall not delay the publication of the details of this diſcovery, from which I claim no credit except on account of the influence which it may have on a part of the commerce of the eaſtern provinces of Spain.

Method of obtaining pure Cobalt. By TROMSDORFF *.

Pure cobalt

LET four parts of finely pulverized zaffre be intimately blended with one of very dry nitrate of potaſh and half a part of charcoal powder; introduce this mixture by ſmall quantities at a time into an ignited crucible, and repeat this proceſs for three ſucceſſive times, by again adding to the detonated reſiduary maſs new quantities of nitrate of potaſh and charcoal. This being done, mix the maſs with one part of black flux, and expoſe the mixture to a red heat for one hour. When cooled, ſeparate the metallic cobalt, pulveriſe it, blend it together with three times its own weight of nitrate of potaſh, and detonate this mixture as directed before. The iron which was preſent will thus became highly oxidized, and the arſenic acidified, and combined with the potaſh. To ſeparate the latter, pulveriſe the maſs, treat it repeatedly in water, and ſeparate the inſoluble part by the filtre. The arſeniate of potaſh being thus got rid of, digeſt the reſidue in nitric acid; the cobalt will now be diſſolved, and the highly oxidized iron remain untouched. Evaporate the ſolution to dryneſs, rediſſolve it in nitric acid, re-filtre the ſolution, in caſe ſome oxide of iron ſhould have eſcaped unſeparated, decompoſe the ſolution of nitrate of cobalt by potaſh, waſh the precipitate, and reduce it by heat.

Method of coating Copper with Platina. By STRAUSS.

Coating copp with platina.

Mr. Strauſs, who has made a number of good experiments on platina, has ſucceeded in applying this metal to defend the face of copper. The ſolution of platina was precipitated by muriate of ammonia, then waſhed and dried, and expoſed to a graduated red heat for half an hour in a covered crucible. The product was a grey coagulated powder; conſiſting of metallic platina in a ſtate of extreme diviſion. One part of this powder with three parts of mercury did not combine by half an hour's trituration; but upon adding two parts more of mercury and ſlightly heating the mortar, he ſoon obtained a tenacious amalgam, which was rendered very ſoft by the addition of two other parts of the laſt mentioned metal.

---

* Gehlen's Journal of Chemiſtry, Vol. IV. p. 6, 117.

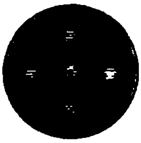
A ſmall quantity of this amalgam was rubbed upon a plate of copper, which became perfectly covered. The plate was then ignited, and was found to have retained a coating of platina. In the next place he mixed a little of the amalgam with chalk, ſprinkled the mixture with water, coated the plate of copper a ſecond time, and again ignited it. The coating was now found to be very perfect, and aſſumed a ſhining ſilver colour under the burniſher.

This chemiſt remarks that his application of platina to copper veſſels muſt be ſuperior to that of tin; not only in its reſiſtance to acids and ſaline matters, but in its durability from the greater hardneſs of platina; and he adds that the proceſs here deſcribed, is not more difficult to be effected than the common operation of tinning.

Tromſdorff's Journal, 1803, Vol. II. p. 18.

Tubular Pendulum by Mr. Edward Troughton.

3



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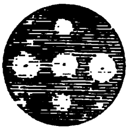

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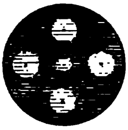

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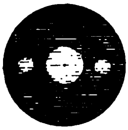

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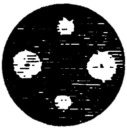

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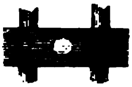

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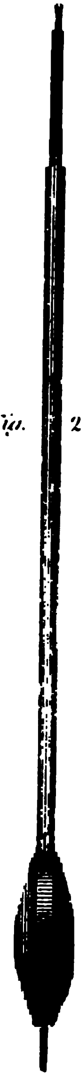

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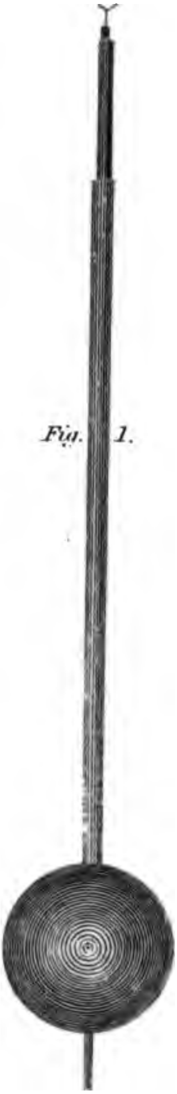

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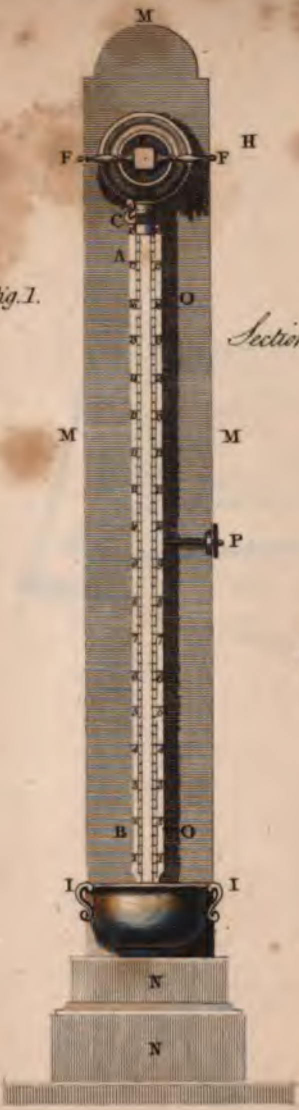

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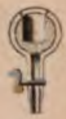

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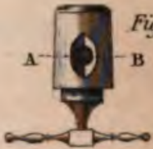

Fig. 2.

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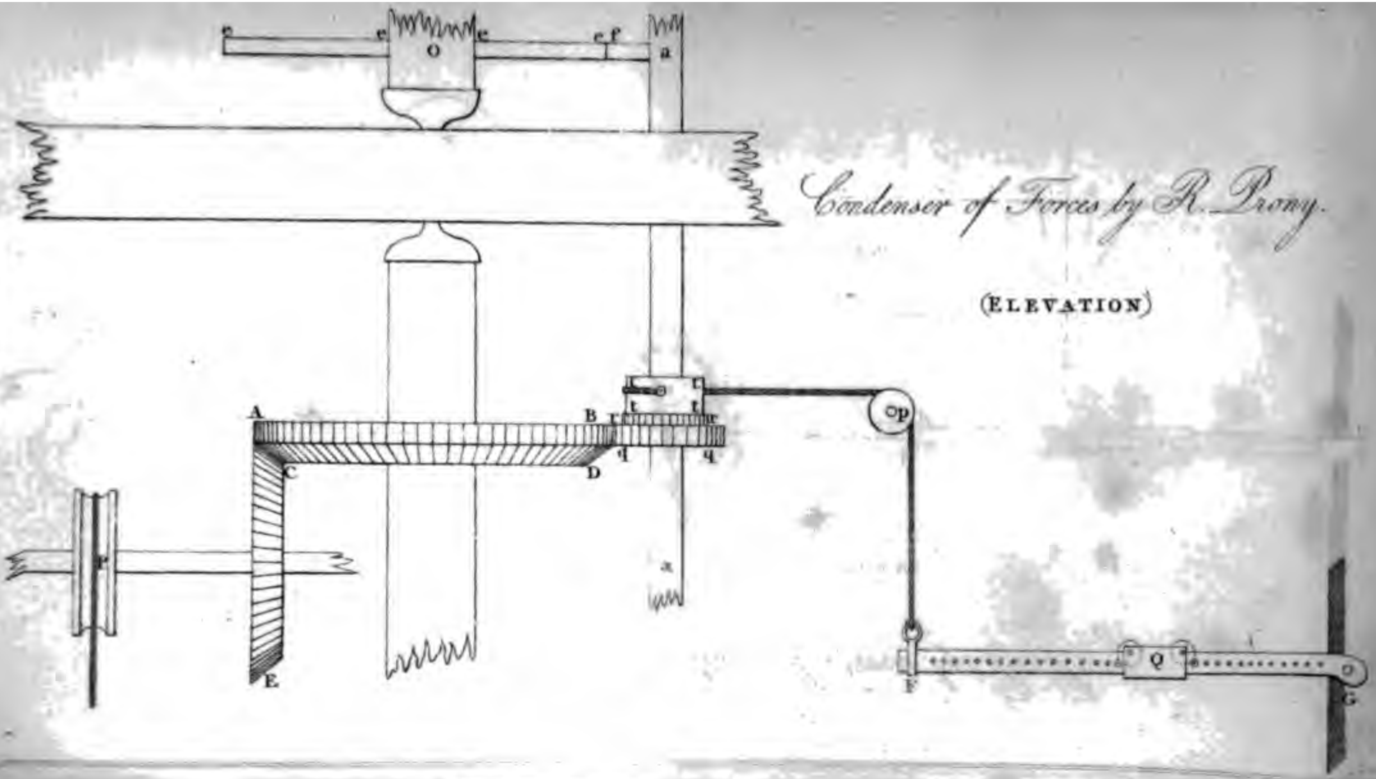
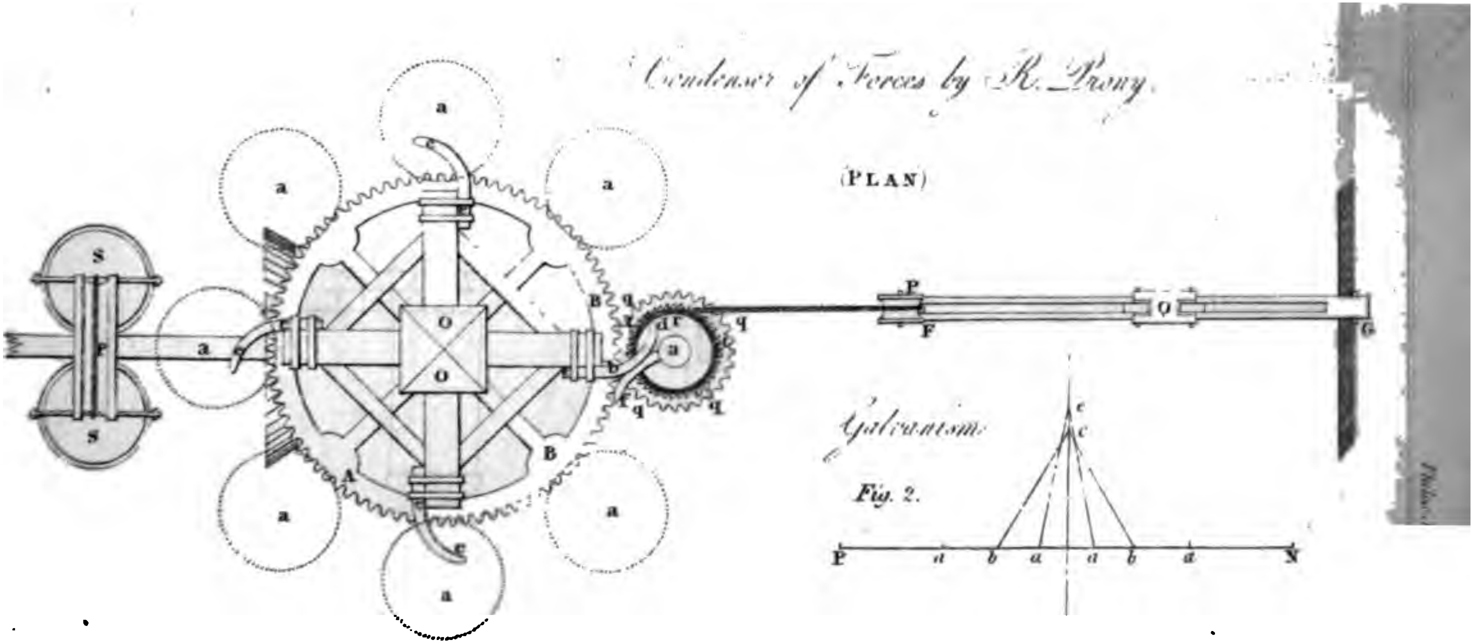


Condenser of Forces by R. Prony.

(ELEVATION)

Condenser of Forces by R. Prony.

(PLAN)



Galvanism.

Fig. 2.

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END OF THE NINTH VOLUME.

ERRATUM.

In *Plate* XII. *Fig.* 3, by an omiſſion in the deſign, the lower ruler has not the ſame connection with the frame as the upper. It muſt be joined by prolonging the rod from the left hand angle, and adding a rod from thence to the bar upon the ruler; as is ſeen above.

Printed by W. Stratford, Crown-Court, Temple-Bar.

A
JOURNAL
OF
NATURAL PHILOSOPHY,
CHEMISTRY,
AND
THE ARTS.

VOL. X. √

Illuſtrated with Engravings.

BY WILLIAM NICHOLSON.

LONDON:
PRINTED BY W. STRATFORD, CROWN COURT, TEMPLE BAR; FOR
THE AUTHOR,
AND SOLD BY HIM AT NO. 10, SOHO-SQUARE;
AND BY
J. MURRAY, NO. 32, FLEET-STREET.

1805.

PREFACE.

THE Authors of Original Papers in the present Volume, are Sir H. C. Englefield, Bart. M. P. F. R. S.; Mr. H. Steinhauer; Mr. John Gough; Ra. Thicknesse, Esq.; Mr. Wm. Wilson; C. Wilkinson, Esq.; J. Whitley Boswell, Esq.; Mr. W. Brandé; Aletes; Mr. John Dalton; Capt. J. Mortlock; W. F. S.; Mr. Charles Sylvester; Mr. Ezekiel Walker; T. B. C.; Jos. Huddart, Esq. F. R. S.; E. O.; W. B.; Mr. N. Mendelſhon; C. L.; Mr. Accum; Right Hon. Sir Joseph Banks, Bart. P. R. S. &c. &c.; Mr. R. Matthews; Mr. J. Biddle; An Old Correspondent.—Of Foreign Works, Thenard; Hesinger; Bergelius; Morozzo; Bucholz; Dartigues; Schoerbing; Boullay; Lagrange; Lussac; Erman.—And of English Memoirs, abridged or extracted; Smithson Tennant, Esq. F. R. S.; W. H. Wollaston, M. D. F. R. S.; C. Hatchett, Esq. F. R. S.; Gregory Watt, Esq.; Right Hon. C. Greville, V. P. R. S.; George Pearson, M. D. F. R. S.; J. Hinckley, Esq.; F. S. A.; T. A. Knight, Esq.

Of the Engravings the Subjects are, 1. An ancient Egyptian Engraving in Symbols, supposed to be astronomical. 2. Figures to illustrate the Galvanic Charge, by C. Wilkinson, Esq. 3. Outlines of Pendulous Measures of Time, by J. W. Boswell, Esq. 4. Figure to illustrate Mr. Gough's Theory of the Augmentation of Sound, by communicated Vibrations. 5. Instrument for surveying, by Capt. J. Mortlock. 6. Instrument for taking Designs from Nature. 7. Plan and Sections of an improved Air-Pump, by Mr. Mendelsshon. 8. Figures to illustrate Mr. Gough's Theory of the Speaking Trumpet. 9. Improvement in Woulfe's Apparatus. 10. Method of drawing a Line in a Circle equal to the Side of its Square. 11. Two quarto Plates, exhibiting highly magnified Representations of the Parasitic Plant which causes the Blight in Corn, from Drawings by M Bauer, Botanical Painter to the King. 12. Mathematical Diagrams of the Properties of Triangles, by Mr. Gough. 13, Application of Spectacle Glasses, by Mr. E. Walker.

Soho Square, April, 1805.

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JANUARY, 1805.

ARTICLE I.

Concerning the original Inventors of certain philoſophical Diſcoveries: the Reflection of Cold; Compreſſion of Water in a metallic Veſſel; the Teleſcope; and a perſpective Inſtrument formerly deſcribed. In a Letter from Sir H. C. ENGLEFIELD, *Bart. M. P. F. R. S. &c. &c.*

To Mr. NICHOLSON.

SIR,

Univerſal ſentiment in favour of diſcoverers.

ALTHOUGH abſtractedly ſpeaking, it is of no importance to ſcience who firſt made the moſt valuable experiments, or to what individuals the world is indebted for the moſt uſeful diſcoveries; yet as the ſenſe of mankind has univerſally in this, as in ſeveral other things, been in direct oppoſition to frigid reaſoning, and every one has been deſirous to vindicate the fame of eminent men, by aſcertaining to them the honour due to their labour or genius, I truſt that the following obſervations will not be deemed unworthy a place in your valuable Journal.

Reflection of cold aſcribed to Pictet.

1. The experiment of the reflexion of cold has been, as far as I know, both in converſation and in printed books, uniformly aſcribed to Mr. Pictet of Geneva. Now in the collec-

It was before made by the Acad. del Cimento.

tion of experiments publiſhed by the Academy Del Cimento, of Florence, in the year 1666, the identical experiment is deſcribed in the following words:

"*Ninth Experiment.*

Narrative.

"We were deſirous to try whether a concave ſpeculum, expoſed to a maſs of ice weighing 500 pounds, would reflect any ſenſible degree of cold on a very delicate thermometer of 400 degrees, placed in it focus. The reſult was, that the thermometer inſtantly ſunk: but a doubt remained whether the thermometer was acted on more by the direct cold of the ice, or that reflected by the ſpeculum. This doubt was removed by covering the ſpeculum: and certain it is (whatſoever might be the cauſe) that the ſpirit inſtantly began to riſe again. Yet ſtill we will not preſume poſitively to affirm, that this riſe might not have been owing to ſome other cauſe than the taking off the reflection from the ſpeculum, all the precautions not having been taken which might be conſidered neceſſary to ſecure abſolute aſſent to the experiment."—*Saggi di Naturali Eſperienze, page* 176.

Excellency of their publication.

I do not mean to aſſert that Mr. Pictet had ſeen this moſt excellent book, or that he borrowed from it without acknowledging his obligation; but the honour of having firſt made the experiment is certainly due to the Florentine philoſophers. It may not be foreign from the ſubject here to obſerve, that the "Saggi," for perſpicuity, brevity, and that diffident caution ſo eſſential to the inveſtigation of truth, is a model of philoſophical writing, not perhaps excelled by any book written ſince that time, and more admirable when we conſider the diffuſe and obſcure ſtile ſo much in faſhion in the works of the learned at that period.

The Acad. del Cimento ſaid, in moſt books, to have compreſſed water in a globe of gold.

The globe was ſilver, and compreſſed by hammering.

2. Scarcely any treatiſe on natural philoſophy has failed to quote an experiment made by the academicians Del Cimento, on the incompreſſibility of water incloſed in a globe of gold, and ſubmitted to the preſſure of a ſcrew preſs. Mr. Canton, in his experiments on the ſame ſubject, publiſhed in the 52d vol. of the Phil. Tranſ. ſo ſpeaks of it. You will probably be ſurpriſed when I aſſert, that the academicians did not try the experiment with a gold globe, but one of ſilver; and that they give a reaſon for uſing ſilver in preference to gold, namely, that the ductility of gold was ſo great that it would

extend its dimenſions by preſſure, and ſo defeat the intention of the experiment. Neither was the globe compreſſed by a machine, but merely by hammering.

Lord Bacon firſt made this experiment.

But the honour of this experiment is not due to the Florentine philoſophers, but to our own illuſtrious countryman Lord Bacon, who, in the *Novum Organum*, Book II. Sect. 45, gives the very ſame experiment on water encloſed in a large and ſtrong globe of lead, which being firſt compreſſed by the hammer and then by a ſcrew-preſs, the water exuded through the pores of the metal, and ſtood in a dew on its ſurface.

The inaccuracy with which this experiment has been related, though in this caſe of no great importance, may however be an uſeful leſſon, and induce thoſe who wiſh to attain to truth, to truſt as little as poſſible to information at ſecond-hand, but to recur to the original authority whenever it is practicable to do ſo.

The teleſcope was uſed by Kepler in aſtronomy, two years before Galileo.

Before I conclude, permit me to mention a curious circumſtance relative to the æra of the invention of the teleſcope. Almoſt all books place it in the year 1609, and the firſt diſcoveries of Galileo were made in 1610; yet Kepler, in his account of the comet which appeared in September, 1607, (*See Kepler de Cometis, page* 25) expreſsly ſtates, that though, to the naked eye, it was only equal to certain fixed ſtars near it, yet, when viewed with teleſcopes (*perſpicillis*), it appeared larger than thoſe ſtars.

Remarks on the progreſs of diſcovery, &c.

Although the intercourſe between the different nations of Europe was at that time much leſs than it now is, it is very difficult to ſuppoſe that Galileo ſhould not, in upwards of two years, have heard, not only of the invention, but its application to celeſtial obſervations by a man ſo eminent in ſcience as Kepler: and it is ſcarcely leſs extraordinary that Kepler himſelf ſhould not, in that ſpace of time, have foreſtalled Galileo in ſome of thoſe diſcoveries which rendered his name ſo illuſtrious; for however imperfect Kepler's teleſcope might be, it is hardly poſſible that it could have failed to ſhew him the ſatellites of Jupiter, which are viſible to the commoneſt opera glaſs.

I am, Sir,
Your obedient ſervant,
H. C. ENGLEFIELD.

Tilney Street, Nov. 30, 1804.

Ramſden was the inventor of a teleſcopic perſpective inſtrument referred to.

P. S. I beg leave to add, that the machine for drawing in perſpective, deſcribed in your Journal for October, page 122, was (I am almoſt abſolutely certain) the invention of the late moſt eminent Mr. Ramſden, and was made by him full 25 years ſince, for the Right Honourable Charles Greville.

II.

Account of an ancient Egyptian Sculpture with Hieroglyphics, ſuppoſed to relate to Aſtronomy. In a Letter from Mr. H. STEINHAUER. *With an Engraving.*

To Mr. NICHOLSON.

SIR, *Fulneck, Oct.* 25, 1804.

Porphyry ſculptured in the form of a ſcarabæus, and bearing hieroglyphics.

PERHAPS the liberty that I take in ſubmitting the annexed draught of an Egyptian antiquity, may be an intruſion upon the plan of your valuable Journal. The Egyptian hieroglyphic inſcriptions are involved in ſuch impenetrable darkneſs, that every trifle contributed to their elucidation cannot but be acceptable, indeed equally acceptable to the antiquarian, the hiſtorian, and the aſtronomer; ſince the greater part have probably a reference to that ſcience. Stones ſculptured in the ſhape of a ſcarabæus are, I believe, (for I make no pretenſions to the title of an antiquarian) well known; there are a few in the Britiſh Muſeum; but I never yet ſaw either a facſimile of the inſcription, or any attempt at an explanation of the ſame. The drawings incloſed are taken from a ſtone brought by the Swediſh conſul at Aleppo to Stockholm, and there preſented by him to a gentleman, who has favoured me with the loan and permiſſion to make drawings or impreſſions of it at pleaſure. The upper part, of the ſize of the ſketch, obviouſly repreſents a ſcarabæus; probably the S. Sacer, Lin. & Fabr. though the indentures on the head are but ſlightly marked, and a ſlight damage between the thorax and elytra render it impoſſible to diſcover whether a ſcutellum be indicated or not. It was perhaps the only beetle they repreſented in ſculpture, and the accuracy of a modern nomenclator is conſequently unneceſſary to determine it. If I recollect right, this beetle was made uſe of as a ſymbol of the annual rotation of the ſun. The under ſide is flat, or ſlightly convex, perhaps from wear, and bears eight lines of hieroglyphics, probably referring to the change of the ſeaſons. The drawing, made with the greateſt accuracy from an impreſſion in ſealing-wax, is conſequently inverted.* Should you, ſir, or any of your friends, wiſh for one to prove the exactneſs of the draught, it is very much at your ſervice. The ſtone itſelf is apparently a ſpecies of porphyry, the *ſerpentino verde antico*, of a green maſs, with very dark green field-ſpar, and pale green indiſtinct ſtripes. The plainneſs of the inſcription, and the ſame ſymbol occurring ſeveral times upon it, led me to ſuſpect, that it would be no very difficult taſk, for ſuch as are verſant in antiquities of the kind, to diſcover ſomewhat of its purport; and being conſcious of its value, I was unwilling to let it lie by me, without offering it to the attention of ſuch as are more deeply intereſted in it. Your wiſh to promote the arts and ſciences, which ſo obviouſly pervades your eſteemed Repoſitory, makes me hope that you will excuſe mine, in caſe the ſubject ſhould not be admiſſible into your Publication.

Porphyry ſculptured in the form of a ſcarabæus, and bearing hieroglyphics.

I remain, Sir,
Your obedient humble ſervant,
H. STEINHAUER.

P. S. Is the carbonate of barytes a frequent Britiſh mineral, except at Chorly? If not, it may perhaps be intereſting information, that it is found in very large quantities near Murton in Cumberland, and ſome other places of the vicinity. The native oxide of zinc, ſcarcely inferior to the flowers of zinc of the ſhops, I do not recollect to have ſeen mentioned. It is found in one part of the Weſt Riding of Yorkſhire, in conſiderable quantities.

Native carbonate of barytes, and oxide of zinc.

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* The engraver having followed the drawing upon the copper, our print is not inverted.—N.

Extract

III.

Extract of a Memoir on the fuming Liquor of CADET.

By Cit. THENARD.*

Hiſtory of the diſcovery of Cadet's fuming liquor, &c.

CADET diſcovered this liquor near half a century ago, in making inveſtigations on arſenic. The name of its author was given to it then, which it has retained ever ſince, becauſe its intimate nature and conſtituent principles were unknown. The thick ſmoke which this ſingular product ſpreads through the air, its ſpecific gravity, greater than that of water, its oily ſtate, its great volatility, its powerful odour, its ſpontaneous inflammation in the air, obſerved by Cadet and the chemiſts of Dijon; in fine, all its properties, each more extraordinary than the other, induced Cit. Thenard to ſubject it to analyſis.

Method of obtaining it. Diſtillation of acetite of potaſh and arſenious acid.

He began by procuring ſeveral ounces of this liquor, by diſtilling, according to Cadet's proceſs, equal parts of acetite of potaſh and arſenious acid, the produce of which he received in glaſs globes, cooled with a mixture of ice and marine ſalt. A liquor, very little coloured, and ſmelling ſtrongly of garlic, ſoon paſſed into the receivers; at the ſame time much gas was diſengaged, which ſpread the ſame odour, and the receivers were filled with vapours, ſo heavy that they ſeemed to run like oil. When the operation was terminated, he unluted the apparatus, and broke the retort. The bottom of it was covered with a white, acrid, and alkaline ſubſtance of potaſh, ariſing from the acetite employed; and the neck was lined with cryſtals of arſenic, owing to the reduction of the arſenious acid. The gaſes, the quantity of which was very great, contained arſeniated hidrogen, in addition to the carbonated hidrogen and carbonic acid, given by all vegetable matters which are decompoſed by fire. The liquid product was compoſed of two very diſtinct ſtrata, holding metallic arſenic in ſolution, which was not long before it was depoſited in flakes; the upper one was of a browniſh-yellow and aqueous, the lower one was leſs coloured, and of an oily appearance. He ſeparated them, by pouring them into a tube drawn to a point by the lamp, which allowed him to receive them

Reſidue.

The liquid product conſiſts of two diſtinct fluids.

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* From Bulletin des Sciences, Tom. III. p. 202.

in

Examination of the heavieſt.

It powerfully affected the animal economy.

in different veſſels. The heavieſt, as being the moſt uſeful to be known, was firſt examined. At the commencement he was affected by the thick vapours which it diſperſes through the air, and by its extremely penetrating and horribly fetid odour. Its action on the animal economy is ſo powerful, that he found it impoſſible to devote more than one hour in a day to his enquiries, and he was more than once tempted to abandon them. He was in the ſame ſtate as if he had taken a ſtrong medicine, and experienced ſtupefying effects; againſt which he employed ſulphurated hidrogen diſſolved in water, with ſucceſs.

Order of the inveſtigation.

As he had but a ſmall quantity of the liquor at his diſpoſal, and it was of importance that his attempts ſhould not be fruitleſs, he regulated the order of his enquiries in this manner: He firſt determined the cauſe of the odour which it ſpreads through the air; then he ſought for that of the thick vapours which it produces, and afterwards that of its ſpontaneous inflammation; and he made uſe of the determination of theſe three points to diſcover the fourth, and moſt important, the conſtituent principles of the ſubſtance.

Cauſe of the odour.

The liquor was unchanged by diſtillation.

The odour could only proceed from the ſubſtance itſelf, or elſe from an elaſtic fluid which it might hold in ſolution, and which the author ſuppoſed to be arſeniated hidrogen. He therefore diſtilled, with great care, a certain quantity of the liquor in a ſmall glaſs retort, to which were adapted a receiver and a tube for collecting the gaſes. He obtained nothing but the air of the veſſels; the liquor was entirely volatilized, and paſſed into the receiver without having undergone any alteration, except that its colour was not quite ſo deep. Hence the odour of the arſenical liquor is owing to its property of being volatilized, and, probably, diſſolved in the air.

Its vapour is occaſioned by an abſorption of oxigen and water.

The cauſe of its vapours could only be owing to an abſorption of oxigen, or to an abſorption of the water diſſolved in the air, or to both theſe effects at the ſame time. The air of a flaſk, into which Cit. Thenard had poured a few drops, immediately loſt its tranſparency, and, in a ſhort time, became incapable of ſupporting the combuſtion of a taper. A veſſel of the ſame ſize, filled with carbonic acid, offered the ſame phenomena, but in a leſs diſtinct manner. To guard againſt the contact of the air, he had been careful to ſuſpend a very thin tube, containing the liquor, to the cork of the flaſk, ſo that he could eaſily break it againſt the ſides of the veſſel. The vapours were not perceptible when he uſed carbonic acid perfectly dried; whence he concluded, that the vapours of the arſenical liquor are owing to an abſorption of the oxigen and the water contained in the air jointly, but that the firſt of theſe cauſes ſeems to be more powerful than the ſecond.

Its property of inflaming ſpontaneouſly is owing to metallic arſenic.

From this it ſhould ſeem that the arſenical liquor is poſſeſſed of the property of inflaming alone. It does not however take fire at the approach of a body in combuſtion, when it is very pure; and it is obſerved, that, in all the ſpontaneous inflammations which it experiences, the combuſtion always begins at the black ſpecks which diſturb its tranſparency, and are only metallic arſenic very much divided.

It muſt therefore contain arſenic,

an oily matter,

and acetous acid;

but this was not indicated by alkalies.

Oxigenated muriatic acid decompoſed it.

Formed a foliated ſalt with potaſh.

Its ſolution in water, decompoſed by ſulphurated hidrogen, yielded ſulphur, arſenic, and oil, containing acetous acid.

It now remained to determine the nature of the arſenical liquor. Its odour, which reſembles arſeniated hidrogen gas, indicated that it muſt contain arſenic, and that this metal muſt have great influence on the phenomena which it offers: Its combuſtibility, its conſiſtence, and its appearance, indicated an oily matter; and, although it did not change the tincture of turnſole, and no re-agent demonſtrated immediately the exiſtence of acetous acid, this body muſt nevertheleſs be expected in it. To ſucceed in inſulating theſe different ſubſtances, the author tried the alkalies; but experience ſoon convinced him that he muſt have recourſe to other means. He employed the oxigenated muriatic acid with much greater ſucceſs. Some drops of the liquor, poured into this gas, were inſtantly inflamed, and their decompoſition was complete. They were then precipitated in white flocks by lime-water, and in yellow ones by ſulphurated hidrogen; while, on being ſaturated with potaſh and evaporated, they formed a foliated ſalt, ſtrongly attracting the humidity of the air, acrid, ſharp, decompoſable by ſulphuric acid, and diſengaging a ſtrong odour of vinegar. The quantity of arſenic and of acetous acid obtained, being far from correſponding with the quantity of liquor which had been uſed, there muſt therefore have been another body in it which it was requiſite to inſulate, and this was accompliſhed by treating a new portion of the liquor with a quantity of water ſufficient to diſſolve it; then on decompoſing it by ſulphurated hidrogen, it yielded a precipitate, ſlightly yellow, very much divided, formed principally of arſenic and ſulphur, which was a conſiderable time in ſeparating from an oil, which was afterwards ſeen ſwimming on the ſurface of the liquor. This contained a great deal of acetous acid. Its decompoſition may be further facilitated by expoſing it to the air; then it emits thick vapours, it is cryſtallized, and becomes ſlightly humid. It is rendered turbid by lime-water, and yields a yellow precipitate with ſulphurated hidrogen.

Compoſition of the heavieſt liquor.

From theſe different experiments it follows, that this liquor is compoſed of oil, acetous acid, and arſenic, nearly in a metallic ſtate; and that it muſt be conſidered as a ſpecies of ſoap, with a baſe of acid and arſenic, or as a ſort of oleo-arſenical acetite.

The upper fluid differs from the lower, only by having a larger proportion of acetous acid and water.

This analyſis was very uſeful in that of the upper liquor. In fact, notwithſtanding the difference which ſeems to exiſt between them, ſince the latter reſembles water, can combine with it in all proportions, forms only a ſlight cloud in the atmoſphere, has much leſs odour, and does not inflame in any caſe, it is eaſy to prove that it only differs from the firſt by its greater proportion of acetous acid, and by the water which it contains; for it reddens the tincture of turnſole ſtrongly, effervesces with the carbonates, gives riſe to acetites, and is ſlightly precipitated of a yellow colour by ſulphurated hidrogen, which ſeparates a little oil from it. A very ſmall quantity of oxigenated muriatic acid ſpeedily deſtroys the odour, and it is then precipitated white by lime-water, and of a deep yellow by the hidro-ſulphurets. Expoſure to the air produces in it, by time, the ſame changes as take place immediately with oxigenated muriatic acid. Finally, a liquor, exactly ſimilar, is formed by diſſolving a few drops of the lower liquor in very weak vinegar, and thus ſynthesis confirms the reſults of analyſis.

Theory of the proceſs.

We can now eſtabliſh a theory, clear of all hypotheſis, on the phenomena offered by the diſtillation of acetite of potaſh and arſenious acid; we ſee that one part of the arſenious acid is entirely reduced; that another is only brought near the metallic ſtate; that the acetite of potaſh is totally decompoſed; that almoſt all the acetous acid is alſo decompoſed; and, that from theſe different decompoſitions reſult water, carbonated hidrogen, arſeniated hidrogen, carbonic acid, a peculiar oil, oxide of arſenic, arſenic, and potaſh; that the potaſh forms the white reſidue found in the veſſels in which the diſtillation

Component parts of the fuming liquor.

was performed; that the arſenic is ſublimed, and adheres to the neck of the retort; that the three different ſpecies of gas are mixed, and may be collected in flaſks; finally, that the water, the oil, the acetous acid, and the oxide of arſenic, are condenſed in the receiver; that theſe three laſt bodies, by combining in certain proportions, form a very volatile compound, heavier than water, and ſparingly ſoluble in it; and that it is for this reaſon that it ſeparates into two very diſtinct ſtrata; the lower of which muſt be conſidered as an oleo-arſenical acetite, and the other as a portion of the firſt diſſolved in water, which ſolution is promoted by an exceſs of acetous acid.

IV.

Account of Cerium, a new Metal found in a mineral Subſtance from Baſtnas, in Sweden. By W. D'Hesinger *and* J. B. Bergelius.

(Concluded from Page 300, Vol. IX.)

With Succinic Acid.

Succinate of cerium.

(P.) At firſt, ſuccinic acid renders the ſaturated ſolutions of cerium turbid, but afterwards they become clear. Thus, a few drops of ſuccinate of ammonia, poured into a ſaturated nitric or muriatic ſolution of this metal, occaſion a precipitate, which ſoon diſappears. On pouring in a greater quantity of this re-agent, the ſuccinate of cerium is immediately depoſited. This combination is white; it is not wholly inſoluble in water, ſince the ſolution from which it was ſeparated ſtill retains a little, as is manifeſted by evaporation, or by alkalies. The ſalt obtained by digeſting free ſuccinic acid with the oxide, comports itſelf in the ſame manner. The acid diſſolves it readily. Expoſed to the fire, it burns with a blue flame.

Since the acetate of cerium is not precipitated by the ſuccinate of ammonia, this is a certain means of obtaining the cerium freed from iron.

With Gallic Acid.

Gallate of cerium.

(Q.) If cryſtallized gallic acid is put into a ſaturated ſolution of muriate of ſerium, a ſmall quantity of a white precipitate

is formed. The alkalies augment it, and give it a clear chocolate colour. If a larger quantity of alkali is gradually added, the quantity of the precipitate, and the intenſity of its colour are increaſed; in proportion as this addition is made, the precipitate becomes of a reddiſh brown, and at length, by expoſure to the light, gives a turbid, deep green ſolution.

With Pruſſic Acid.

Pruſſiate of cerium.

(R.) The ſaturated ſolutions of cerium are precipitated white by the pruſſiates. The voluminous precipitate has much reſemblance to ſilver precipitated by muriate of ſoda. An exceſs of acid readily diſſolves this precipitate.

Oxide of Cerium with Sulphurated Hidrogen.

Hydro-ſulphuret of cerium.

(S.) At the commencement, the ſaturated ſolutions of cerium are precipitated of a browniſh colour by hidro-ſulphuret of ammonia; but if more of this re-agent is added, the precipitate is of a deep green. The muriate of cerium alone takes a deep green colour, but if an alkali is added to it, an hidro-ſulphurated combination of muriate of cerium, at a *minimum* of oxidation, is formed, which is rapidly precipitated of a bright green colour.

The hidro-ſulphurated cerium, well dried, has a deep green colour, almoſt black. It is eaſily deſtroyed by heat; put into a crucible, moderately hot, it burns with a yellowiſh, phoſphoric flame, only viſible in the dark.

Sulphurated Oxide of Cerium.

Sulphurated oxide of cerium.

(T.) The ſulphuret of ammonia gives a fading brown precipitate with the ſolutions of cerium. This ſulphuret, added in exceſs, gives a precipitate of a graſs green, which becomes bright green by deſiccation: it burns with a blue flame and the pure oxide remains.

The muriate of cerium at a *minimum* is precipitated white by ſulphuret of ammonia.

Oxide of Cerium with Phoſphorus.

Phoſphate of cerium.

(U.) A piece of pure phoſphorus was put into a ſolution of muriate of cerium contained in a cloſed veſſel, and kept for ſeveral days on a ſtove. The bottom and ſides of the veſſel were covered with a white precipitate, and the piece of phoſ-

phorus was covered with a hard brown cruſt, from which the phoſphorus was ſeparated by heating it in warm water. This cruſt was tenacious, and ſhining in the dark. Heated, it took fire like phoſphorus, and left a reſidue of a ſmall quantity of oxide, which, on being melted with borax, exhibited the characters deſcribed above, except that the globule retained its clear green colour, after being cooled. It appears probable that theſe phenomena aroſe from the phoſphorus containing cerium. But the phoſphorus, kept for a month in the ſame ſolution, ſlowly depoſited a white powder; whence it is uncertain whether the formation of the cruſt, mentioned above, was owing to the cerium. The acid was always in exceſs in the ſolution, and the white powder had all the appearance of phoſphate of cerium.

With the Alkalies.

Action of the alkalies.

(V.) The pure alkalies do not diſſolve cerium, even by fuſion. By this means it is eaſily deprived of manganeſe. Pure ammonia digeſted with the oxide, does not diſſolve it, but renders it yellowiſh.

The carbonated alkalies diſſolve the oxide of cerium in ſmall quantity. The ſolution is yellow, and is precipitated by the acids. They alſo diſſolve it by fuſion in covered veſſels. In the open fire, it oxides too much to enter into ſolution.

Iron or zinc do not precipitate the ſolutions of cerium.

The ſolutions of cerium are not precipitated by iron or zinc.

Action of galvaniſm.

The electric pile of Volta only decompoſes the ſalts, and the yellow oxide adheres to the conductor.

V.

New Experiments on Abſorption by Charcoal, made by Means of a new Machine. By C. L. Morozzo.

(Concluded from P. 264 Vol. IX.)

On Oxigen Gas.

Oxigen gas.

25th. I NEXT proceeded to the examination of the abſorption by charcoal in oxigen gas.

I therefore extracted ſome oxigen gas from red precipitate to be employed in my different experiments. Of this gas,

trie

tried with charcoal in my machine, eight inches and ſix lines were abſorbed: this great abſorption ſurprized me; for in my firſt experiments, in 1783, made in cloſed tubes. I had had but a very ſmall one: I therefore found no difficulty in attributing it to my matraſs having been melted towards the cloſe of the operation, which made me ſuſpect that my gas had been ſpoiled by the nitrous acid; and I repeated the experiment with great care. Oxigen gas.

26th. I then took oxigen gas which had been obtained from red precipitate with great attention, and had been prepared in the laboratory of Doctor Bonvoiſin; what was my aſtoniſhment at obſerving that, in thirty-ſix hours, the abſorption amounted to twelve inches and eight lines, and that it continued for ſeveral days: in forty-eight hours it was fourteen inches ſix lines; in four days, fifteen inches two lines; in five days, ſixteen inches four lines; and, finally, in three days more, the gas was entirely abſorbed.

27th. This experiment was too intereſting to omit repeating it with the greateſt poſſible preciſion.

I therefore took ſome of the ſame oxigen gas, and the abſorption was twelve inches three lines in the firſt four hours; in forty-eight hours, it was thirteen inches ſix lines; in three days, fourteen inches five lines; at the end of the fourth day, it was fifteen inches three lines? of the fifth, ſixteen inches; of the ſixth, ſixteen inches ſeven lines; of the ſeventh, ſeventeen inches two lines; finally, on the eighth day the gas was entirely abſorbed.

28th. I repeated the ſame experiment on oxigen gas obtained from water expoſed to the ſun, in which I had put three ounces of charcoal-powder*; in this gas, which was very pure, the abſorptions took place in the ſame manner.

29th. Theſe facts will ſurpriſe thoſe who, having made their experiments on oxigen gas in tubes cloſed hermetically, and by paſſing the charcoal through the mercury, obtained only very ſmall abſorptions, which never exceeded three inches

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* In the twelfth volume of the Italian Society, I have inſerted a memoir on the property which charcoal mixed with water has of developing one-third more of oxigen gas, of a ſuperior quality to that obtained from pure water: In this memoir I prove, that a part of this oxigen gas is produced by the charcoal, which communicates the *principle of fire* to the air contained in the water.

three

Oxigen gas.

three lines in tubes of twelve inches in height. I am certainly not miſtaken; and M M. Rouppe and Van-Norder, who have repeated my experiments in their apparatuſes, have obtained the ſame reſults, and have found, as I did, that, next to hidrogen, oxigen gas is that which is leaſt abſorbed by charcoal.*

30th. I muſt not paſs over a ſingular experiment, the explanation of which will alſo be very difficult.

I examined the abſorption which a piece of charcoal, that had remained ſeven hours in the bright light of the ſun, had effected in the ſame oxigen gas; it was only between ſeven and eight lines, while a ſimilar piece, which had been only five hours in the ſolar light, produced an abſorption of ten inches three lines in carbonic acid gas.

It muſt be obſerved, that a piece of charcoal expoſed to the light of the ſun and placed in oxigen gas, produced only an abſorption of ſeven or eight lines, juſt like the abſorption of a piece which had been expoſed to the ſun's light, and placed in hidrogen gas, which gave only an abſorption of ſix lines: and, in this, theſe reſults are perfectly conformable to my experiments, made in the year 1783.

31ſt. This difference of the reſults is therefore only owing to the method either of paſſing the charcoal under the mercury, or of leaving it in the machine. It appears that, by paſſing it through the mercury, the charcoal loſes much of its attractive power, which it retains in the machine.

Let us endeavour to find whether theſe differences can be accounted for.

32d. It might be ſuppoſed that the red-hot charcoal inflames the oxigen gas; but ſince I do not open the key until ſome time after the charcoal has been inſerted, it does not appear to me to be likely that the gas can be inflamed: beſides, if this were the caſe, the abſorption would be made at once, and it would not require eight days to be complete.

33d. It might be ſuppoſed that, in this caſe, the charcoal ſupplied hidrogen gas, which, mixing with the oxigen gas and producing water, would cauſe the abſorption: but during the whole experiment there was not the ſmalleſt drop of water perceived in the tube.

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* Ann. de Chimie.

34th. There might be reaſon to believe that the charcoal, by inflaming the oxigen gas, might convert it into carbonic acid gas, which is the moſt abſorbed of all the gaſes; but to ſatisfy myſelf of this, I produced an abſorption of oxigen gas by a piece of charcoal, as in the former experiments: in twenty-three hours it amounted to twelve inches, then with a ſyringe of cryſtal I paſſed ſome lime-water through the mercury into the remaining gas, and it did not become turbid, which proves that carbonic acid was not formed; and, having turned up the tube, the reſidual gas extinguiſhed a taper: I believe it to have been azote.

35th. I was of opinion that incandeſcent charcoal would abſorb more than another piece which had been ſuffered to cool till it no longer appeared red. Experiment ſhews a greater abſorption in the firſt for twenty-four hours; but, leaving the apparatus undiſturbed, in two days the abſorption became equal. I operated upon atmoſpheric air.

36th. I muſt acknowledge my inability to give an explanation of theſe ſingular experiments, as I have already declared above: were I to hazard any conjectures, they would probably be overturned by new theories, which do not ſpare even the labours of the greateſt chemiſts in France.

37th. I ſhall therefore continue my operations as I propoſed, and new experiments will, at leaſt, ſupply the want of ſyſtems.

On the Difference in the Charcoals made uſe of.

Difference in the charcoals made uſe of.

38th. Having aſcertained the abſorptions effected by the charcoal of beech wood in atmoſpheric air and in the different gaſes, I proceeded to examine the variations which the charcoals of different woods would produce in them.

Charcoal of ſallow;

39th. I therefore took two pieces of charcoal, the one of beech, the other of the branches of ſallow deprived of bark: I introduced them both in an incandeſcent ſtate, and examined the difference of their abſorption in the ſame atmoſpheric air.

That of beech gave an abſorption of ſeven inches and eight lines in ſix hours.

That of ſallow gave only an abſorption of four inches and three lines, and there was no further abſorption after ſix hours.

Theſe experiments correſpond perfectly with thoſe which I have publiſhed in my third memoir. I ſhall diſpenſe with the relation of ſeveral other experiments which I have made on different woods, ſuch as hazle, vine-twigs, &c. all of which gave leſs abſorptions.

f box;

I afterwards tried the charcoal of box wood, although it is a very compact wood; but ſince Kirwan found that this wood contains much more ſaline matter than any other wood, it was deſirable to ſubmit it to experiment: its abſorption, however, was conſiderable, and equal to that produced by beech-wood. The greater or leſs quantity of ſaline matter in the wood of which the charcoal is made, therefore, does not contribute to the variations in the abſorption.

f cork.

40th. I afterwards examined the charcoal of cork, which intereſted me greatly, becauſe Cit. Odier, from the experiments of Dr. Beddoes, has propoſed it as a medicine: I was deſirous of examining whether it contained more of the principle of fire than the others. I therefore charred cork; but it is ſo light, that, after the charring, a piece of equal bulk with that of the beech which I had uſed, weighed only three grains: to bring the circumſtances to an equality, it was therefore neceſſary to make the experiment with a piece of beech charcoal which weighed three grains*: it was in atmoſpheric air it was tried. The following are the reſults:

Three grains of beech charcoal produced an abſorption of one inch and nine lines.

The charcoal from cork abſorbed only nine lines.

The charcoal of cork contains more of the principle of fire than any other charcoal.

41ſt. According to my principles†, the charcoal of cork muſt contain the matter of fire more than any other charcoal; thus when it is neceſſary to give it as a medicine, either inwardly or outwardly, it ſhould be preferred to all the others; and I am of opinion that it is by oxidating, and not by diſoxidating, that it produces its aſtoniſhing effects. Beſides, we have inſtances of ſubſtances which act in this manner. The oxigenated muriatic acid, far from abſtracting the oxigen, is ſuppoſed, on the contrary, to add a conſiderable quantity of it to fetid ulcers, and carries off their fetidity as well as charcoal.*

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* The pieces of beech charcoal which I uſe, and which weigh thirty-ſix grains, are of the length of ten lines, and about five in diameter.

† See my third memoir on charcoal.

Water may ſometimes be uſed in the Inſtrument inſtead of Mercury.

Coloured water may ſometimes be ſubſtituted to mercury in the machine.

42d. Although, in the courſe of theſe experiments, I have conſtantly employed mercury, in ſome experiments a coloured water may be uſed with advantage, ſince it is better ſeen in the tube; but this cannot be uſed when the gaſes which are abſorbed by water are examined, nor with the carbonic acid and the other acid gaſes which are much abſorbed: then it is neceſſary to change the tube, and to ſubſtitute one which, inſtead of eighteen inches, is at leaſt thirty-ſix in height; without which it will be entirely filled.

43d. It was on atmoſpheric air taken in my room with the windows open, that I experimented with coloured water. The charcoal produced an abſorption of ten inches and a half. It has been ſeen, that, with the mercurial apparatus, I had had an abſorption of eight inches ſome lines.

The preſſure of the atmoſphere affects the reſults.

The preſſure of the atmoſphere in the water therefore augmented the abſorption, which uſually takes place with the mercury, by two inches and a half; that is to ſay, one-fourth of the height.

44th. I ſhould obſerve, that very frequently the ſmall differences of one or two lines obſerved in the abſorptions of the ſame quality of air or of gas, are owing to the variations of the atmoſphere, which I aſcertained by comparing the height of the barometer during the experiment.

The Pieces of Charcoal uſed in theſe Experiments, acquire Weight.

The charcoal acquires weight in theſe experiments;

45th. After the experiments, all the pieces of charcoal employed had acquired greater weight: this augmentation was from half a grain to two grains, and appears to be dependent on the greater abſorption.

46th. I took four pieces of charcoal which had been uſed in the experiments made with atmoſpheric air, which, among them, had acquired four grains in weight: I paſſed them and yields a portion of the abſorbed air, which is a compound of azote and carbonic acid gaſes;

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* See the notes of M. Odier to Beddoes's memoir. Bibl. Brit. Vol. VI. p. 359.

under water, in a tube of one inch in diameter and ſix inches high, which was alſo filled with water. A part of the air which they contained was immediately developed, but I only obtained about two inches, (they had, however, abſorbed among them nearly thirty-two inches). The reſtored air was found to be compoſed of carbonic acid gas and azote gas. In this operation a part of the air is fixed in the charcoal to be embodied with it, and cannot be again diſlodged but by employing fire. I believe theſe combinations frequently occur in nature, and the great Newton ſuſpected it.

by which it regains its abſorbent power.

47th. I dried the pieces of charcoal which had been wetted in the preceding experiment, in the air: I afterwards examined them, by again heating them to redneſs in the fire, and placing them in the machine; they abſorbed the ſame quantity of air as before.

No water produced by the diminution of the gas under examination.

48th. I directed all my attention, as well after as during the courſe of the experiments on atmoſpheric air, to ſee if I could perceive any drops of water either in the tube or in the apparatus, but I did not obſerve the leaſt trace of it; neither was the charcoal moiſt; on the contrary, its ſurface was covered with aſhes.

Experiments to be made by ſaturating the Charcoal with different Subſtances.

Experiments ſtill to be made.

49th. Since, during the progreſs of an inveſtigation, there are always new experiments which offer themſelves, I perceived that it would be intereſting to make trial with pieces of charcoal ſaturated with different ſaline and acid ſubſtances, as M. M. Rouppe and Van-Norden, of Rotterdam, had undertaken to do, and to ſee the effects they would produce, with my machine, as well in atmoſpheric air as in the different gaſes: But my memoir being of ſufficient length already, I leave the care of theſe enquiries to other philoſophers; and this the more willingly, ſince it appears that one of the moſt able chemiſts, M. Van Mons, has engaged in this ſubject, which, in his hands, will doubtleſs receive every degree of perfection which can be deſired.

50th. I ſhall only give the reſult of four experiments which I made with atmoſpheric air.

I ſaturated pieces of charcoal with nitric acid, ſulphuric acid, ſolution of potaſh, and lime-water; they were not uſed until they had been thoroughly dried on filtering-paper. I put them cold into my machine. The following are the reſults:

1ſt. In the firſt ſix hours the charcoal, ſaturated with nitric acid, produced an abſorption of four lines and a half; afterwards it gave back the air, and even depreſſed the mercury below its level.

Effects of charcoal ſaturated with nitric acid;

2d. The charcoal, ſaturated with ſulphuric acid, effected an abſorption of nine lines, which it preſerved uniformly for twenty-four hours.

with ſulphuric acid;

3d. The charcoal, ſaturated with ſolution of potaſh, made an abſorption of three lines, and remained ſo for twenty-four hours.

with ſolution of potaſh; and

4th. That, ſaturated with lime-water, made only a ſmall abſorption of a line and a half, although it was kept in experiment for twenty-four hours.

with lime-water.

Theſe four experiments prove that charcoal, ſaturated with different ſubſtances, loſes, in part, its power of abſorbing: probably theſe ſolutions, by filling the pores of the charcoal, in ſome meaſure deprive it of this property.

Charcoal loſes its abſorbent power by being ſaturated with ſome ſubſtances.

51ſt. No perſon can be more aware than I am, that my enquiry is far from being carried to that degree of perfection which I wiſhed: I hope, nevertheleſs, that philoſophers will not be diſſatisfied that I have preſented them with new experiments, and have made them acquainted with an inſtrument, by means of which they can appreciate the effects of ſolar light and caloric on charcoal; an inſtrument which, in many caſes, will ſerve them for an eudiometer, and which, when azote ſhall be better known, will acquire greater preciſion. I hope that, by the labours of other philoſophers who may employ themſelves on this ſubject, this intereſting branch of natural philoſophy will be brought to perfection, and an explanation of theſe ſurpriſing phenomena be obtained.

VI.

Farther Obſervations on the Conſtitution of mixed Gaſes. In a Letter from Mr. JOHN GOUGH.

To Mr. NICHOLSON.

SIR,

An apology for this letter.

OTHER philoſophers, beſides Mr. Dalton, have maintained the exiſtence of free elaſtic vapour in the atmoſphere; but as the modification given by this gentleman to the opinion, is perhaps the laſt it will receive, I am deſirous of treſpaſſing once more on your pages, in reply to my opponent, as well as to ſhew the changes which the preſent controverſy has produced in his ſyſtem.

Some objections ſtated by Mr. D. conſidered.

Mr. Dalton's laſt letter begins by expoſing my ignorance of chemiſtry, and diſapproving the data, by help of which air has been ſhewn to be a chemical compound. Although his ſuſpicions may be juſt in many inſtances, they are wrong in this; for the facts ſtated by Mr. D. were known to me at the time of writing that paper; and the reprobated data were adopted for the following reaſons:

Specific gravity of oxigenous gas.

Mr. Davy found the weight of 100 cubic inches of oxigenous gas, of the temperature of 50°, to be 35.06 grains, the barometer ſtanding at 30 inches: now the denſities of the ſame elaſtic fluid at 50° and 60°, are in the ratio of 105824 to 103744*: but when the magnitude is given, *i. e.* 100 parts, the weights are as the denſities; conſequently the weight of 100 cubic inches of oxigenous gas at 60°, is 34.37 grains, the barometer ſtanding at 30: but 31 grains being the weight of an equal bulk of air in like circumſtances, and its ſpecific gravity being denoted by 1000, the correſponding ſpecific gravity of oxigenous gas is expreſſed by the number 1108; which exceeds Mr. Kirwan's expreſſion only by five. This approximation to an exact coincidence, affords the ſtrongeſt evidence of the correctneſs both of Mr. Kirwan and Mr. Davy, that can be expected in a queſtion of the kind; and, as the former gentleman made uſe of the ſame apparatus to determine the weight of air which had loſt its oxigen by being expoſed to the martial paſte of ſulphur, the ſpecific gravity, drawn from his experiment, was certainly to be preferred in my calculation to any other. This reſiduum was called by me azote, probably becauſe Mr. Kirwan called it phlogiſticated air: but it was evidently conſidered as a compound in the eſſay alluded to by Mr. Dalton; and certainly the advocate for a multiplicity of co-exiſtent atmoſpheres will not controvert the ſuppoſition. The remaining objections made by my opponent to the paper in queſtion, are levelled at my ignorance, not at my concluſions; they ſhall therefore be paſſed over in ſilence. In the mean time, Mr. D. perhaps will not take it amiſs if I requeſt him to repeat his calculations with the ſpecific gravity of oxigenous gas when properly corrected, and to try what will be the variation of the eudiometer, either on the data of Mr. Davy or M. Lavoiſier.

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* Thomſon's Chemiſtry, Vol. I. p. 342.

A new inſtrument recommended.

Permit me, Mr. Nicholſon, to conclude this part of my ſubject by obſerving, that an inſtrument of great uſe in pneumatics might be conſtructed on the principles of your hydroſtatic weighing machine. The body of it ſhould be of glaſs, furniſhed with proper ſtopples and ſtop-cocks, ſo that it might be eaſily exhauſted and repleniſhed. The preceding is but an imperfect ſketch of an inſtrument, the mechaniſm of which I reſign to your pen and pencil, ſhould the idea appear worth purſuing.

A miſrepreſentation pointed out.

Mr. Dalton's ſtrictures on my laſt letter aſcribe to me very ſingular opinions; but he neglects to point out the paragraph where the grounds of this charge may be found; a charge which inſinuates, that, according to my notions of things, a veſſel containing a cubic foot of one gas cannot receive an equal bulk of a different kind: Here indeed he accuſes me of groſs ignorance; for there are but few who do not know that fermenting liquors increaſe the denſity of the gaſes, which occupy part of the cloſe veſſels containing them. In reality, the charge is ſo perfectly groundleſs, that, had it been made by almoſt any other perſon, I would not have heſitated to have pronounced my accuſer guilty of miſrepreſentation to conceal his want of argument, and to ſecure the prejudices of the ſuperficial reader in his own favour. He may court the admiration of ſuch; I do not deſire their applauſes.

Mr. D.'s important argument no argument at all.

Mr. Dalton ſpeaks with confidence of what he calls his important argument, *i. e.* his aſſertion, that equal quantities of vapour enter a vacuum and an equal ſpace previouſly occupied by a gas. No one would refuſe its due importance to this argument, after ſeeing or peruſing a ſatisfactory demonſtration; but I know of no ſuch thing in Mr. Dalton's eſſays. He aſſerts, indeed, that a quantity of dry air, confined in a tube, receives an augmentation of force from the preſence of water, equal to that which is produced by the ſame cauſe in the exhauſted tube. But this is no demonſtration, unleſs the poroſity of air be firſt admitted; for, without this conceſſion, Mr. D. has no right to eſtimate the quantity of vapour preſent by the ſpring of it; this ought to be determined by weight only. But when did Mr. D. weigh an exhauſted veſſel, and afterwards take its weight when filled with vapour? When has he repeated the ſame experiment upon the ſame veſſel containing dry and moiſt air, and found the reſult favourable to his hypotheſis? Negative anſwers to theſe queſtions muſt convince an impartial judge, that the poroſity of the air has been again artfully introduced to eſtabliſh its own exiſtence: thus we find the fundamental datum of the ſyſtem to be demonſtrated by arguing in a circle, which begins and ends with the thing to be proved.

On the other hand, Mr. D. is convinced that the admiſſion of vapour into an open bottle expels part of the air. This is a proof that ſteam, ſuppoſing its exiſtence in the atmoſphere, finds a ſpace crowded with gaſes leſs convenient than a vacuum; it, therefore, makes room for itſelf, by diminiſhing the denſities of theſe gaſes, and creates in them Mr. Dalton's capacity for the reception of water. This fact being eſtabliſhed, it will alſo be evident, that, when vapour is forced into a gas confined in a cloſe veſſel, the former cannot make its way through the latter, otherwiſe than by condenſing it: Such is the nature of the pores of the air, according to Mr. D.'s own principles. The atmoſphere is alſo found to oppoſe a ſimilar reſiſtance to newly developed gaſes; for the gaſeous compound contained in an open veſſel filled in part with a fermenting liquor, is not a column of the atmoſphere permeated by an additional body of the carbonic acid, but a mixture, in which the proportion of common air is comparatively ſmall. This fact overturns Mr. D.'s general concluſion, that atmoſpheres may be added to the compound atmoſphere at pleaſure,

ſure, or taken from it, without its diſturbing the denſities and ſituations of the remaining atmoſpheres*.

Change in Mr. D.'s opinions.

The free paſſage of gaſes through each other was once the fundamental maxim of my opponent; at which time he maintained, that the variations in the weight of the atmoſpheric compound ariſe from the changeable nature of the aqueous atmoſphere, not from the permanent gaſes †. This dogma is eſſential to the ſyſtem; it has, however, been abandoned by its author himſelf; for Mr. D. when attempting to explain the obſervation of Mr. Kirwan, is obliged to admit the permanent gaſes to be more abundant in dry than in moiſt air; that is, the denſities and poſitions of theſe gaſes depend upon the ſtate of the contained vapour, contrary to the hypotheſis. This departure from the primitive maxim is avowed openly in Mr. D.'s explanation of the experiment with the moiſt bottle. Here he is compelled to confeſs, that ſteam, at low temperatures, diſtends the pores of air; and endeavours to preſerve the exiſtence of his aqueous atmoſphere by ſomething, which looks like a demonſtration from the final Q. E. D.: but it ought to be remembered, that the particles of a fluid preſs equally in all directions; conſequently, that the corpuſcles of air would act with their full force on the contiguous corpuſcles of his ſuppoſed vapour.

Perhaps Mr. D. will perceive, by this time, that logical preciſion has placed him in the circumſtances of certain philoſophers mentioned in his third eſſay, who are unable to defend their opinions. In fact, Mr. D.'s hypotheſis is repugnant to natural appearances in its primitive form; for, according to it, a ſhower of rain is a prodigy; ſeeing the drops muſt diſplace equal bulks of air, and this removal muſt be brought about by the inadequate weight of a column of vapour which has loſt part of its ſpring.

JOHN GOUGH.

Middleſhaw, December 13, 1804.

P. S. Since the publication of my paper on vegetation, I have found, by Dr. Thomſon's Chemiſtry, that M. Ingenhouz had formerly made the ſame diſcovery. In 1795, I read his work, *Sur les Vegetaux*, printed at Paris, 1787; but undoubtedly this ſection had by ſome means been overlooked. All that I have to do at preſent, by way of reparation, is to thank Dr. Thomſon for his information, and to declare that paper of no value; which in all probability will prove a ſufficient apology to M. Ingenhouz.

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* Mancheſter Memoirs, Vol. V. p. 546.

† Mancheſter Memoirs, Vol. V. p. 547.

VII.

On two Metals, found in the black Powder remaining after the Solution of Platina. By SMITHSON TENNANT, *Eſq. F.R.S. From the Philoſophical Tranſactions for* 1804.

Introduction, with reference to the experiments made by Deſcotils,

UPON making ſome experiments, laſt ſummer, on the black powder which remains after the ſolution of platina, I obſerved that it did not, as was generally believed, conſiſt chiefly of plumbago, but contained ſome unknown metallic ingredients. Intending to repeat my experiments with more attention during the winter, I mentioned the reſult of them to Sir Joſeph Banks, together with my intention of communicating to the Royal Society, my examination of this ſubſtance, as ſoon as it ſhould appear in any degree ſatisfactory. Two memoirs were afterwards publiſhed in France, on the ſame ſubject; one of them by M. Deſcotils, and the others by Meſſrs. Vauquelin and Fourcroy. M. Deſcotils chiefly directs his attention to the effects produced by this ſubſtance on the ſolutions of platina. He remarks, that a ſmall portion of it is always taken up by nitro-muriatic acid, during its action on platina; and, principally from the obſervations he is thence enabled to make, he infers, that it contains a new metal, which, among other properties, has that of giving a deep red colour to the precipitates of platina.

and by Vauquelin.

M. Vauquelin attempted a more direct analyſis of the ſubſtance, and obtained from it the ſame metal as that diſcovered by M. Deſcotils. But neither of theſe chemiſts have obſerved, that it contains alſo another metal, different from any hitherto known.

Subſtance obtained from the grains of platina,

The ſubſtance with which my experiments were made, was obtained from platina which had been previouſly freed from the ſand and other impurities generally mixed with it; ſo that it muſt have been contained in the ſubſtance of the grains of platina. Though it has ſomewhat the appearance of plumbago, it may eaſily be diſtinguiſhed by its ſuperior weight. By weighing it in a phial with water, I found its ſpecific gravity almoſt 10.7.

reſembling plumbago, but heavier.

It combines with metals:—A ſmall portion renders lead, biſmuth, zinc, and tin, difficultly fuſible

Before I deſcribe the method of ſeparating the two metals of which it conſiſts, it may be worth while to mention the effects of it, when combined with different metals in its entire ſtate. It readily unites with lead; but, even with ten times its own weight, the compound has not, when melted, much fluidity. Upon diſſolving the lead in nitrous acid, the black powder was obtained, with little apparent alteration, not having been entirely broken down, but conſiſting chiefly of the ſame ſcaly particles as at firſt. With biſmuth, zinc, and tin, the effects were nearly ſimilar; but, by fuſion with copper in a very ſtrong heat, a more perfect union was produced. On attempting to diſſolve the compound by nitro-muriatic acid, me of the powder was taken up with the copper, forming a very dark ſolution.

with copper;

ſilver or gold; not ſeparable by refining.

The undiſſolved portion conſiſted partly of the ſubſtance in its original form of ſcales, and partly of a blacker powder, the particles of which were too ſmall to be viſible, and which had probably been completely combined with the copper. This ſubſtance may be eaſily united, by fuſion, with ſilver or gold; and it is particularly deſerving of attention, that it cannot be ſeparated from theſe metals, by the uſual proceſs of refining. It remains combined with either of them, after cupellation with lead; and with the gold, after quartation with ſilver. The alloys retain conſiderable ductility; and the colour of that with gold, is not materially different from pure gold.

Analyſis of the black powder; which conſiſts of two metals.

Fuſion with ſoda.

Solution in water, deep orange, leaving a powder;

which was partly diſſolved in muriatic acid.

I ſhall now proceed to deſcribe the analyſis of the black powder, and the properties of the two metals which enter into its compoſition. The method which I uſed for diſſolving it, was ſimilar to that employed by M. Vauquelin, the alternate action of cauſtic alkali, and of an acid. I put a quantity of the powder into a crucible of ſilver, with a large proportion of pure dry ſoda, and kept it in a red heat for ſome time. The alkali being then diſſolved in water, had acquired a deep orange, or browniſh-yellow colour, but much of the powder remained undiſſolved. This powder, digeſted in marine acid, gave a dark blue ſolution, which afterwards became of a duſky olive-green, and finally, by continuing the heat, of a deep red colour. Part of the powder being yet undiſſolved by the marine acid, was heated as before with alkali; and, by the alternate action of the alkali and acid, the whole appeared capable of ſolution. At each operation, ſome ſilex was taken up by the alkali; and, as this continued till the metallic part was entirely diſſolved, it ſeems to have been chemically combined with it.

Fuſion, &c. repeated on the undiſſolved reſidue.

Silex.

The alk. ſolution contains an oxide which ſeparates by repoſe.

The alkaline ſolution contains the oxide of a volatile metal, not yet noticed, but which I ſhall preſently deſcribe, and alſo a ſmall proportion of the other metal. If this ſolution is kept for ſome weeks, the latter metal ſeparates ſpontaneouſly from it, in the form of very thin flakes, of a dark colour.

The acid ſol. contains the ſame metal, but principally that examined by Vauquelin.

The acid ſolution alſo contains both the metals, but principally that which has been mentioned by the French chemiſts. The properties of this laſt metal, which they have remarked, are thoſe of giving a red colour to the triple ſalt of platina with ſal-ammoniac, of not being altered by muriate of tin, and of giving, with pure alkali, a dark brown precipitate. M. Vauquelin alſo adds, that it is precipitated by galls, and by pruſſiate of potaſh; but I ſhould rather aſcribe theſe precipitates to ſome impurity, and probably to iron.

The name *Iridium* given to the metal examined by Vauquelin.

As it is neceſſary to give ſome name to bodies which have not been known before, and moſt convenient to indicate by it ſome characteriſtic property, I ſhould incline to call this metal *Iridium*, from the ſtriking variety of colours which it gives, while diſſolving in marine acid.

In order to obtain the compound of this metal with marine acid in a pure ſtate, I tried to make it cryſtallize.

Cryſtals of its muriate

diſſolved in water.

By ſlow evaporation of the ſolution, only an imperfectly cryſtallized maſs was produced; but this, being dried on blotting-paper, and diſſolved in water, afforded, by again evaporating as before, diſtinct octaedral cryſtals. Theſe cryſtals diſſolved in water, gave a deep red coloured ſolution, inclining to orange. With an infuſion of galls no precipitate was formed, but the colour was inſtantly, and almoſt intirely, taken away. Muriate of tin, carbonate of ſoda, and pruſſiate of potaſh, produced nearly the ſame effect. Pure ammonia precipitates the oxide; but (poſſibly from adding it in exceſs) I found it retained a part in ſolution, acquiring a purple colour. The pure fixed alkalis alſo precipitate the greater part of the

Precip. by alkalis and metals.

oxide, but are capable of retaining a part in ſolution, becoming of a yellow colour. All the metals which I tried, excepting gold and platina, produced a dark or black precipitate from the muriated ſolution, which is at the ſame time deprived of its colour. The iridium may be obtained in a pure ſtate, merely by expoſing the octaedral cryſtals to heat, which expels the oxigen and the muriatic acid. It appeared of a white colour, and was not capable of being melted, by any degree of heat I could apply. I could not combine it with ſulphur, nor with arſenic. Lead eaſily unites with it; but is ſeparated by cupellation, leaving the iridium upon the cupel, as a coarſe black powder. Copper forms with it a very malleable alloy, which, after cupellation with the addition of lead, left a ſmall proportion of the iridium, but much leſs than in the former caſe. Silver may be united with it, and the compound remains perfectly malleable. The iridium was not ſeparated from it by cupellation, but occaſioned on the ſurface a dark or tarniſhed hue. It appeared not to be perfectly combined with the ſilver, but merely diffuſed through the ſubſtance of it, in the ſtate of a fine powder. Gold alloyed with iridium is not freed from it by cupellation, nor by quartation with ſilver. The compound was malleable; and did not differ much in colour from pure gold, though the proportion of alloy was very conſiderable. If the gold or ſilver is diſſolved, the iridium is left, in the form of a black powder.

The metal iridium is white, infuſible, and rejects ſulphur and arſenic.

Its habitudes with the metals.

The yellow alkaline ſolution, which I have already mentioned as containing a metallic oxide, diſtinct from the former, is conſidered by M. Vauquelin as a ſolution of the oxide of chrome in alkali; but I could not, by any teſt, diſcover the preſence of chrome. After the ſuperfluous alkali had been neutralized by an acid, it produced a pale or buff-coloured precipitate with a ſolution of lead, and not the bright yellow which is given by chrome. But, as we are indebted to the above diſtinguiſhed chemiſt, among many other important diſcoveries, for our knowledge of the exiſtence of chrome, it is not improbable that ſome kinds of platina may contain that ſubſtance, beſides the other bodies uſually mixed with it. When the alkaline ſolution is firſt formed, by adding water to the dry alkaline maſs in the crucible, a pungent and peculiar ſmell is immediately perceived. This ſmell, as I after-

The alkaline ſolution did not contain *Chrome.*

It contains the volatile oxide of a metal called *Oſmium*;

wards diſcovered, ariſes from the extrication of a very volatile metallic oxide; and, as this ſmell is one of its moſt diſtinguiſhing characters, I ſhould on that account incline to call the metal *Oſmium.*

expellable from the alkali by an acid;

This oxide may be expelled from the alkali by any acid, and obtained in ſolution with water by diſtillation. The ſulphuric acid, being the leaſt volatile, is the moſt proper for this purpoſe; but as, even of this acid, a little is liable to paſs over, a ſecond ſlow diſtillation is required, to obtain the oxide perfectly free from it. The ſolution thus procured is without colour, has a ſweetiſh taſte, and the ſtrong ſmell before mentioned. Paper ſtained blue with violets, was not changed by it to red; but, by being expoſed to the vapour of it in a phial, the paper loſt much of its blue colour, and inclined to gray. As a certain quantity of this oxide is extricated during the ſolution of the iridium in marine acid, that part may alſo be obtained by diſtillation.

or obtained by diſtilling the black powder with nitre.

Another mode by which the oxide of oſmium may be obtained in ſmall quantity, but in a more concentrated ſtate, is, by diſtilling with nitre the original black powder procured from platina.

Proceſs deſcribed.

With a degree of heat hardly red, there ſublimes into the neck of the retort, a fluid apparently oily, but which, on cooling, concretes into a ſolid, colourleſs, ſemitranſparent maſs. This, being diſſolved in water, forms a ſolution ſimilar to that before deſcribed. The oxide, in this concentrated ſtate, ſtains the ſkin of a dark colour, which cannot be effaced.

Moſt ſtriking character of ſolution of oſmium:

The moſt ſtriking teſt of the oxide of oſmium, is an infuſion of galls, which preſently produces a purple colour, becoming ſoon after of a deep vivid blue. By this means, the preſence of this, and of the metal firſt deſcribed, may be obſerved,

Habitudes with various bodies;

when the two are mixed together. The ſolution of iridium is not apparently altered by being mixed with the oxide of oſmium; but, on adding an infuſion of galls, the red colour of the firſt is inſtantly taken away, and ſoon after the purple and blue colour of the latter appears. The ſolution of the oxide of oſmium with pure ammonia, becomes ſomewhat yellow, and ſlightly ſo with carbonate of ſoda. It is not affected by pure magneſia, nor by chalk; but with lime a ſolution is formed, of a bright yellow colour. The ſolution with lime gives with galls a deep red precipitate, which becomes blue by acids. It produces no effect on a ſolution of platina or gold; but precipitates lead of a yellowiſh-brown, mercury of a white, and muriate of tin of a brown colour.

—alcohol;

The oxide of oſmium becomes of a dark colour with alcohol, and after ſome time, ſeparates in the form of black films, leaving the alcohol without colour. The ſame effect is produced by ether, and much more quickly.

— the metals.

This oxide appears to part with its oxygen to all the metals, excepting gold and platina. Silver being kept in a ſolution of it for ſome time acquires a black colour; but does not entirely deprive it of ſmell. Copper, tin, zinc, and phoſphorus, quickly produce a black or gray powder, and deprive the ſolution of all ſmell, and of the power of turning galls of a blue colour. This black powder, which conſiſts of the oſmium in a metallic ſtate and the oxide of the metal employed to precipitate it, may be diſſolved in nitro-muriatic acid, and then becomes blue with infuſion of galls.

Amalgam with mercury.

If the pure oxide of oſmium, diſſolved in water, is ſhaken with mercury, it very ſoon loſes its ſmell; and the metal, combining with the mercury, forms a perfect amalgam.

Pure oſmium had by diſtilling off the mercury.

Much of the mercury may be ſeparated by ſqueezing it through leather, which retains the amalgam of a firmer conſiſtence. The remaining mercury being diſtilled off, a powder is left, of a dark gray or blue colour, which is the oſmium in its pure ſtate. By expoſing it to heat with acceſs of air, it evaporates, with the uſual ſmell; but, if the oxidation is carefully prevented, it does not ſeem in any degree volatile. Being ſubjected to a ſtrong white heat, in a cavity made in a piece of charcoal, it was not melted, nor did it undergo any apparent alteration. Heated in a ſimilar ſituation with copper and with gold, it melted with each of theſe metals, forming alloys which were quite malleable. Theſe compounds were eaſily diſſolved in nitro-muriatic acid, and, by diſtillation, afforded the oxide of oſmium with the uſual properties.

It is gray, and not fuſible; nor volatile, unleſs oxided.

Alloys.

Pure oſmium not ſoluble in acids; but eaſily with alkali.

The pure metal which has been previouſly heated, does not ſeem to be acted on by acids; at leaſt I could not perceive any effect produced by boiling it for ſome time with nitro-muriatic acid. By heating it in a ſilver cup with cauſtic alkali, it immediately combined with the alkali, and, with water, gave a yellow ſolution, ſimilar to that from which it was procured. Acids expel from this ſolution the oxide of oſmium, which has the uſual ſmell, and the power of giving to infuſion of galls the blue colour before mentioned.

VIII.

Remarks upon certain Obſervations of Mr. WILKINSON, *reſpecting Galvaniſm. By* RA. THICKNESSE, *Eſq.*

To Mr. NICHOLSON.

SIR, *Wigan, Dec.* 17, 1804.

Obſervations reſpecting galvaniſm, &c.

THE remarks of Mr. Wilkinſon (which I have not had an earlier opportunity of attending to) on my letter of the 20th September, on galvaniſm, are in general founded only on miſconception; the conſequence, probably, of a haſty peruſal in a public room. He attributes to me an aſſertion, "that two metals are requiſite to the production of galvanic phenomena;" but my words are, and particularly marked by italics, "two metals, *or other ſubſtances;*" and were intended to ſignify two metals, or two other ſubſtances; or one metal and one other ſubſtance; and to include even that which may be diſſolved in the water. The experiment, therefore, of La Grave, with brain and muſcle, which Mr. W. adduces, is not an exception; and "that a ſingle metal ſuffices" perfectly with pure water only, he did not, I fancy, wiſh to be underſtood.

That copper and the other negative metals are, as I expreſſed, "acted upon by the hidrogen," I muſt refer to Mr. W. for authority, to pages 177 and 178 of your Journal; and that particularly ſilver (my words are copper, or ſilver) is rendered more brittle, to page 85: but there cannot, ſurely, be ſtronger evidence that copper has an affinity for hidrogen, and conſequently an influence in the decompoſition of the water, than is afforded by the experiments in which hidrogen gas is obtained from the copper of the pile, and oxigen gas from the oxidated zinc; part of the hidrogen, and of the oxigen, being prevented from uniting with, and borne away from the metals by the electricity.

Mr.

Obſervations reſpecting galvaniſm, &c.

Mr. W. imputes to me alſo the miſtake of having "obſerved that the ſenſation is in proportion to the ſurface acted on;" adding, that "the experiments of the French philoſophers proved, that the action of galvaniſm on animal ſubſtances is in the ratio of the number of plates employed, and not the ſurfaces expoſed." This is another inſtance of the inaccuracy of Mr. W.'s obſervations; for ſenſation I never mentioned; nor action on animal ſubſtances; not at all conſidering the intenſity, but the quantity only of the electric fluid produced; which may not always correſpond; for the charge of ſmall plates being weak, although according to their ſurfaces, the electricity meets with almoſt inſurmountable obſtructions from the interſtices of the pile; which to a ſtronger charge, from larger plates, are very trifling impediments: but Mr. W. thinks, himſelf, that "the production of galvanic phenomena is always proportionate to the degree of oxidation;" and muſt not the quantity of metal oxidated be in proportion to the ſurface expoſed to the water? The French philoſophers, too, have "concluded (I quote Mr. W.'s own words, from p. 207 of your laſt volume) that the effects of a galvanic battery on metallic ſubſtances, are in proportion to the ſurfaces of the plates employed:" Muſt we not then ſuſpect the experiments to have been imperfect which led them to conclude, if they have ſo concluded, that the effects on animal ſubſtances are different?

Whether I "ſet out too hypothetically as to electricity being a modification of caloric," is a queſtion on which many of the moſt celebrated men of ſcience would differ from Mr. W. in opinion. "*Pars invenit utraque Cauſſas*; yet it appears to me, that the experiment noticed by *a Correſpondent*, p. 173, almoſt determines it: "If the gas which is produced from one of the wires communicating with the pile in the water, be united and inflamed with the other in a juſt proportion, the water which is common to both is reproduced, and *common fire* in great abundance." Thus the electric fluid, which alone conveys the oxigen and hidrogen from the water, actually becomes, when differently modified by combuſtion, for there is "no evidence of the preſence of fire until this point of time," active caloric.

But ſuppoſing, with Mr. W. that the electric matter and caloric are perfectly diſtinct bodies, we have ſtill the ſame reaſon

Obſervations reſpecting galvaniſm, &c.

reaſon to conclude that the former is combined with water, which we have to believe that it is contained in metals: What then becomes of it when the water is decompoſed, if, as he imagines, the electric, or "galvanic phenomena, entirely depend upon the metal undergoing a chemical change," I muſt confeſs I cannot conjecture: but, in truth, it is entirely the cauſe of all the phenomena which take place: the quantity afforded by the metal, ſo very ſmall a portion of which undergoes any chemical change, muſt be too trifling to be conſidered; for if any more than the oxidated part of the metal contributed, the quantity of the metal in the plates, as well as the extent of its ſurface, would in ſome degree have an effect: but this, as I alledged, is not the caſe; and, therefore, Mr. W.'s fanciful conjecture, that there is as it were a ſpring of electric fluid in the metal, as a reſource for a ſupply, although very ingenious, may be regarded as a perfect hypotheſis—a ſuppoſition unſupported by any fact; and as an inſtance, from his partiality for it on this occaſion, of our diſpoſition to overlook imperfections in our own performances, whilſt we obſerve them in the works of others.

I am, Sir,

Your moſt obedient ſervant,

RA. THICKNESSE.

IX.

Abſtract of a Memoir on the Poſſibility of obtaining Pruſſiate of Potaſh free from Iron; the Unalterability of the Pruſſic Acid at high Temperatures; and the true Nature of the Combinations of this Acid with different Baſes. By BUCHOLZ.

(Concluded from p. 282, Vol. IX.)

General deductions.

THESE two experiments therefore prove, that from four parts of dried blood and one of carbonate of potaſh, no more pruſſiated alkali can be obtained than from two of blood and one of alkali.

From what has been ſo far ſtated the author concludes, after varying and repeating the experiments here detailed,

1. That four parts of blood to one of carbonate of potaſh are too much, and that equal parts of blood and carbonate of potaſh are rather good proportions for obtaining the moſt poſſible quantity of pruſſiated alkali. The latter proportions have, moreover, the diſadvantage, that the exceſs of alkalis acts upon the oxide of iron of the blood as well as upon the crucible, and therefore introduces new impurities. Two parts of blood and one of carbonate of potaſh, appear to be a better proportion for obtaining the greateſt poſſible quantity of pruſſiate of potaſh, under like circumſtances.

Beſt proportion of carbonate of potaſh and dried blood, are 1 p. and 2 b.

2. Alcalies deprived of carbonic acid, are not better than carbonated alkalies for the production of pruſſiated alkalies by treatment with blood; for they become reconverted into carbonated alkalies during the proceſs.

It is no advantage to uſe a pure alkali.

3. No pure pruſſiate of potaſh can be obtained, except care be taken that the mixture of blood and alkali be neither too much nor too little ignited. The criterion which may ſerve as a guide to the operator, is the ceſſation of the flame, after a gradually increaſed ignition without fuſion. For if the maſs be heated ſuddenly ſo as to effect a partial fuſion, or if it be continued after the diſappearance of the flame, it will be in vain to look for pure pruſſiated alkali.

The formation of pure pruſſiate requires a gradual heat without fuſion, and no longer than till the flame ceaſes.

4. The high colour of pruſſiated alkalis is not always owing to the preſence of iron, but more frequently to a minute quantity of charcoal. This impurity is not capable of yielding reſults that could miſlead the chemical enquirer in the application of this re-ägent.

When the high colour of p. alk. ariſes from charcoal, it does not produce fallacy.

5. The quantity of water employed for the lixiviation of pruſſiated alkali from the ignited maſs, ſhould be as ſmall as poſſible, and heat ſhould be avoided as much as poſſible. For if pruſſiated alkalis be heated in contact with water, part of the pruſſic acid is liberated, ammonia is produced, and carbonate of potaſh formed.

Very little water ſhould be uſed, and heat avoided.

6. Acetic acid cannot be employed for removing the admixture of carbonate of potaſh from pruſſiated alkalis; for the energy of the affinity of pruſſic acid is leſs than the energy of the affinity of carbonic acid to potaſh; the union of the pruſſic acid and potaſh is therefore demoliſhed, in preference to that of the carbonate of potaſh.

Acetic acid cannot be uſed to take off the redundant potaſh.

X.

On a new Metal, found in Crude Platina. By WILLIAM HYDE WOLLASTON, *M. D. F. R. S. From the Philoſophical Tranſactions*, 1804.

Introduction.

NOTWITHSTANDING I was aware that M. Deſcotils had aſcribed the red colour of certain precipitates and ſalts of platina, to the preſence of a new metal; and although Mr. Tennant had obligingly communicated to me his diſcovery of the ſame ſubſtance, as well as of a ſecond new metal, in the ſhining powder that remains undiſſolved from the ore of platina; yet I was led to ſuppoſe that the more ſoluble parts of this mineral might be deſerving of further examination, as the fluid which remains after the precipitation of platina by ſal ammoniac, preſents appearances which I could not aſcribe to either of thoſe bodies, or to any other known ſubſtance.

New metal, *Rhodium*.

My inquiries having terminated more ſucceſsfully than I had expected, I deſign in the preſent Memoir to prove the exiſtence, and to examine the properties, of another metal, hitherto unknown, which may not improperly be diſtinguiſhed by the name of *Rhodium*, from the roſe-colour of a dilute ſolution of the ſalts containing it.

Palladium found in ore of platina.

I ſhall alſo take the ſame opportunity of ſtating the reſult of various experiments, which have convinced me, that the metallic ſubſtance which was laſt year offered for ſale by the name of Palladium, is contained (though in very ſmall proportion) in the ore of platina.

Remaining ſolution after precipitation of platina, contains iron and other metals.

The colour of the ſolution that remains after the precipitation of platina, varies, not only according to its ſtate of dilution, but alſo according to the ſtrength and proportions of the nitric and muriatic acids employed. This colour, though principally owing to the quantity of iron contained in it, ariſes alſo in part from a ſmall quantity of the ammoniaco-muriate of platina, that neceſſarily remains diſſolved, and from other metals contained in ſtill ſmaller proportions.

Precipitable by zinc or iron.

(A. 1.) To recover the remaining platina, as well as to ſeparate the other metals that are preſent from the iron, I have in ſome experiments employed zinc, in others iron, for their precipitation. The former appears preferable; but, when the latter has been uſed, the precipitate may immediately be freed from the iron that adheres to it, by muriatic acid, without the loſs of any of thoſe metals which are at preſent the ſubject of iniquiry.

The ſame reſidual ſolution affords cryſtals by ſpontaneous evap.

(A 2.) Having in one inſtance diſſolved ſuch a precipitate in nitro-muriatic acid, and precipitated the platina by ſal ammoniac, I ſuffered the remaining fluid to evaporate without heat; and obtained a mixture of various cryſtals, very different from each other in form and colour. From theſe, I ſelected for examination ſome that were of a deep red colour, partly in thin plates adhering to the ſides of the veſſel, and partly in the form of ſquare priſms having a rectangular termination.

Deep red cryſtals left a white metal by heat,

—not platina.

(A 3.) A portion of theſe cryſtals being heated in a ſmall tube, yielded ſal ammoniac by ſublimation, and left a black reſiduum, which, by greater heat, acquired a brilliant metallic whiteneſs, but could not be fuſed under the blowpipe. Having obtained this ſubſtance from a diſtinctly cryſtallized ſalt, I was inclined to conſider it as a ſimple metal; and, as I found it to be wholly inſoluble in nitro-muriatic acid, I judged it not to be platina.

The cryſtals were not of platina, nor that which makes the ſalts of platina red.

(A 4.) The cryſtals alſo, inſtead of being nearly inſoluble, like the ammoniaco-muriate of platina, were diſſolved in a ſmall quantity of water, and gave a roſe-coloured ſolution. Upon mixing this with a ſolution of platina, the ammonia was transferred by ſuperior affinity to the latter, forming an ammoniaco-muriate of platina; and the precipitate was of a yellow colour. Conſequently, the metal contained in the ſalt, was neither platina nor that which gives the red colour to the ſalts of platina.

The baſe was the metal rhodium now firſt noticed.

It would be uſeleſs to detail my firſt unſucceſsful experiments made upon the properties of this metal, in hopes of diſcovering means by which its ſeparation from platina might be effected; I ſhall therefore confine myſelf to the following proceſs, which appears to be the moſt direct for procuring rhodium in a ſtate of purity. In the ſame proceſs alſo palladium is obtained, ſo as to afford a preſumption, that it is rather a natural ſimple body, than any artificial compound.

Proceſs for obtaining it.

Purification of the platina-grains.

(B 1.) Since the platina to be procured in this country, generally contains ſmall ſcales of gold intermixed, as well as a portion of the mercury which the Spaniards employ for the ſeparation of the gold, the platina uſed for my experiments, after being by mechanical means freed, as far as poſſible, from all viſible impurities, was expoſed to a red heat, for the purpoſe of expelling the mercury. It was then digeſted for ſome time in a ſmall quantity of dilute nitro-muriatic acid, and frequently ſhaken, till the whole of the gold was diſſolved, together with any impurities that might ſuperficially adhere to the grains of platina.

Solution of the ore in nitro-mur. acid.

(B 2.) Of the ore thus prepared, nearly 2½ ounces were then diſſolved in nitro-muriatic acid, (diluted for the purpoſe of leaving as much as poſſible of the ſhining powder,) and the whole ſuffered to remain in a moderate ſand heat, till completely ſaturated.

Precipitation by ſal amm.

(B 3.) Such a portion of this ſolution was then taken for analyſis, as correſponded to 1000 grains of the prepared ore. An ounce of ſal ammoniac was next diſſolved in hot water, and uſed for the precipitation of the platina. The precipitate obtained was of a yellow colour, and, upon being heated, yielded 815 grains of purified platina.

Reſidual liquor precipitated by zinc.

(B 4.) The water uſed for waſhing this precipitate having been added to the ſolution poured from it, a piece of clean zinc was immerſed in it, and ſuffered to remain, till there appeared to be no further action upon the zinc. The iron contained in the ore (to the amount of 14 or 15 *per cent.*) remained in ſolution. The other metals had ſubſided, in the form of a black powder, which I eſtimated between 40 and 50 grains; but, as there was no occaſion to weigh it with accuracy, I thought it better not to dry this precipitate, for, if it be heated, the rhodium is in danger of being rendered inſoluble.

Precipitate deprived of copper and lead by nitric acid; and then diſſolved (moſt part) in nitro-m. acid.

(B 5.) As I had previouſly aſcertained that this precipitate would contain platina, rhodium, the ſubſtance called palladium, copper, and lead, the two laſt metals were firſt diſſolved in very dilute nitric acid, aided by a gentle heat. The remainder, after being waſhed, was digeſted in dilute nitro-muriatic acid, which diſſolved the greater part, but left as much as 4½ grains undiſſolved.*

Addition of common ſalt and evap. left ſoda-muriates of platina, palladium and rhodium. All but the triple ſalt of rhodium was waſhed off by alcohol.

(B 6.) To the ſolution were added 20 grains of common ſalt; and, when the whole had been evaporated to dryneſs with a very gentle heat, the reſiduum, which I had found, from prior experiments, would conſiſt of the ſoda-muriates of platina, of palladium, and of rhodium, was waſhed repeatedly with ſmall quantities of alcohol, till it came off nearly colourleſs. There remained a triple ſalt of rhodium, which by theſe means is freed from all metallic impurities.

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* It was preſumed that this reſiduum conſiſted principally of the metal called by Mr. Tennant Iridium; but, as it was accidentally miſlaid, and was not examined, it might alſo contain a portion of rhodium.

(C 1.) This ſalt, having been diſſolved in a ſmall quantity of hot water, and let to ſtand 12 hours, formed rhomboidal cryſtals, of which the acute angle was about 75°.

Solution in water.

(C 2.) It was then again diſſolved in water, and divided into two equal portions. Of theſe, one was decompoſed by a piece of zinc, and the other examined by the following re-agents.

One half precip. by zinc.

(C 3.) Sal ammoniac occaſioned no precipitation; but, when a ſolution of platina was added to the mixture, a precipitate was immediately formed, and the colour of this precipitate was yellow; which again proves that the metal contained in this ſalt, is neither platina itſelf nor that which gives the red colour to its precipitates.

The other half was precip. by platina, but not by ſal-amm.

(C 4.) Pruſſiate of potaſh occaſioned no precipitation, as it would have done, if the ſolution had contained palladium.

nor by pr. potaſh,

(C 5.) Hydro-ſulphuret of ammonia, which would have precipitated either platina or palladium, cauſed no precipitation of this metal.

nor by hydro-ſulph. of amm.

(C 6.) The carbonates of potaſh, of ſoda, or of ammonia, occaſioned no precipitation; but the pure alkalis precipitated a yellow oxide, ſoluble by exceſs of alkali, and alſo ſoluble in every acid that I have tried.

nor by carb. alkalis; but by pure alk. ſoluble in acids.

(D 1.) The ſolution of this oxide in muriatic acid, upon being evaporated, did not cryſtallize; the reſiduum was ſoluble in alcohol, and of a roſe colour. Sal ammoniac, nitre, or common ſalt, cauſed no precipitation from the muriatic ſolution; but formed triple ſalts, which were not ſoluble in alcohol.

Muriatic ſolution of rhodium.

(D 2.) The ſolution in nitric acid alſo did not cryſtallize. A drop of this ſolution, being placed upon pure ſilver, occaſioned no ſtain. On the ſurface of mercury a metallic film was precipitated, but did not appear to amalgamate. The metal was alſo precipitated by copper and other metals, as might be preſumed, from the uſual order of their affinities for acids.

Nitric ſolution.

(E 1.) The precipitate obtained by zinc (C 2.) from the remaining half of the ſalt, appeared in the form of a black powder, weighing, when thoroughly dried, nearly 2 grains, correſponding to about 4 grains in the 1000 of ore diſſolved.

The precip. before made by zinc was black,

(E 2.)

ſimply heated,

(E 2.) When expoſed to heat, this powder continued black; with borax, it acquired a white metallic luſtre, but appeared infuſible by any degree of heat.

fuſible with arſenic and with ſulphur.
Metallic button not malleable,

(E 3.) With arſenic, however, it is, like platina, rendered fuſible; and, like palladium, it may alſo be fuſed by means of ſulphur. The arſenic, or the ſulphur, may be expelled from it by a continuance of the heat; but the metallic button obtained does not become malleable, as either of the preceding metals would be rendered by ſimilar treatment.

unites with metals except mercury.

(E 4.) It unites readily with all metals that have been tried, excepting mercury; and, with gold or ſilver it forms very malleable alloys, that are not oxidated by a high degree of heat, but become incruſted with a black oxide, when very ſlowly cooled.

Gold and rhodium 4 to 1.

(E 5.) When 4 parts of gold are united with one of rhodium, although the alloy may aſſume a rounded form under the blowpipe, yet it ſeems to be more in the ſtate of an amalgam than in complete fuſion.

Gold and rhodium 6 to one.
It does not diſcolour gold.

(E 6.) When ſix parts of gold are alloyed with one of rhodium, the compound may be perfectly fuſed, but requires far more heat than fine gold. There is no circumſtance in which rhodium differs more from platina, than in the colour of this alloy, which might be taken for fine gold, by any one who is not very much accuſtomed to diſcriminate the different qualities of gold. On the contrary, the colour of an alloy containing the ſame proportion of platina, differs but little from that of platina. This was originally obſerved by Dr. Lewis. "The "colour was ſtill ſo dull and pale, that the compound (five "to one) could ſcarcely be judged by the eye to contain any gold."*

I find that palladium reſembles platina, in this property of deſtroying the colour of a large quantity of gold. When one part of palladium is united to ſix of gold, the alloy is nearly white.

Solubility of rhodium affected by combination.

(E 7.) When I endeavoured to diſſolve an alloy of ſilver or of gold with rhodium, the rhodium remained untouched by either nitric or nitro-muriatic acids; and, when rhodium had been fuſed with arſenic or with ſulphur, or when merely heated by itſelf, it was reduced to the ſame ſtate of inſolubility. But, when one part of rhodium had been fuſed with three parts of biſmuth, of copper, or of lead, each of theſe alloys could be diſſolved completely, in a mixture of two parts, by meaſure, of muriatic acid, with one of nitric. With the two former metals, the proportion of the acids to each other ſeemed not to be of ſo much conſequence as with lead; but the lead appeared on another account preferable, as it was moſt eaſily ſeparated, when reduced to an inſoluble muriate by evaporation. The muriate of rhodium had then the ſame colour and properties, as when formed from the yellow oxide precipitated from the original ſalt. (D 1.)

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* Lewis's Philoſophical Commerce of Arts, p. 526.

Specific gravity.

(E 8.) The ſpecific gravity of rhodium, as far as could be aſcertained by trial on ſo ſmall quantities, ſeemed to exceed 11. That of an alloy conſiſting of one part rhodium and about two parts lead, was 11,3; which is ſo nearly that of lead itſelf, that each part of this compound may be conſidered as having about the ſame ſpecific gravity.

The alcohol before uſed in waſhing the ſalts examined.

F. As it was expected that the alcohol employed for waſhing the ſalt of rhodium (B 6.) would contain the ſoda-muriates of platina and of palladium, the platina was firſt precipitated by ſal ammoniac. This precipitate was of a deep red colour; and, when it had been heated, to expel the ſal ammoniac, the platina which remained was of a dark gray colour.

(G 1.) To the remaining ſolution, after it had been diluted to prevent any further precipitation of platina, I added pruſſiate of potaſh, which inſtantly occaſioned a very copious precipitate, of a deep orange-colour at firſt, but changing afterwards to a dirty bottle-green, which I aſcribed to iron contained in the pruſſiate.

(G 2.) This precipitate, when dry, weighed 12½ grains. After it had been heated, it left a metallic reſiduum, in ſmall grains, of a gray colour, weighing nearly 7 grains. A ſmall portion of it being heated with borax, communicated a dark brown colour to the borax, as from iron, and acquired a bright metallic luſtre, but could not be fuſed under the blowpipe. With ſulphur, however, it fuſed immediately into a round globule, which, by floating upon mercury, appeared of leſs ſpecific gravity than that metal.

(G 3.) The whole quantity was then treated in the ſame manner, and purified by cupellation with borax, till it cooled with a bright ſurface. From the globule the ſulphur was expelled,

pelled, by expoſure to the extremity of the flame; and it became ſpongy and malleable, weighing in this ſtate very nearly five grains.

Palladium.

(G 4.) A portion of this metal was diſſolved in ſtrong nitrous acid, was precipitated by green ſulphate of iron, and in other reſpects, poſſeſſed all the properties aſcribed to the palladium offered for ſale, in the printed paper that accompanied it, as well as others ſince noticed by Mr. Chenevix.

Its differences from platina.

(G 5.) In its precipitation by pruſſiate, it differs moſt eſſentially from platina; and conſequently is by no means difficult to be diſtinguiſhed, or ſeparated from it.

(G. 6.) The action of muriate of tin upon the ſolutions of theſe metals, is alſo totally different. A dilute ſolution of platina, is thereby changed from a pale yellow to a tranſparent blood-red. A ſolution of palladium, on the contrary, uſually becomes opaque, by the formation of a brown or black precipitate; but, if mixed in ſuch proportion as to remain tranſparent, it changes to a beautiful emerald-green.

(G 7.) In the formation of triple ſalts with the alkalis, as obſerved by Mr. Chenevix, palladium may be ſaid to reſemble platina; but the ſalts thus formed are far more ſoluble than the correſponding ſalts of platina, and differ entirely, in the colour and form of the cryſtals.

(G 8.) The ſoda-muriate of palladium is a deliqueſcent ſalt; that of platina, on the contrary, forms permanent cryſtals.

(G 9.) The triple ſalts of platina, with either muriate of ammonia or of potaſh, form octaedral cryſtals of a yellow colour, that are very ſparingly ſoluble in water. The correſponding ſalts of palladium, likewiſe reſemble each other in every reſpect. The cryſtals are very ſoluble in water, but inſoluble in alcohol; their form is that of a four-ſided priſm, and they each preſent a curious contraſt of colour, that certainly is not obſervable in any known ſalt of platina.

Curious contraſt of colour in the ſalts of palladium.

(G 10.) Although the ſolution is of a deep red, the cryſtals are of a bright green when viewed tranſverſely. In the direction of their axes, however, the colour is the ſame as that o the ſolution; but, on account of its extreme intenſity, it is with difficulty diſtinguiſhed in fragments that exceed $\frac{1}{100}$ of an inch in thickneſs. One conſequence of this colour is nevertheleſs very obſervable; namely, that in viewing any cryſtal obliquely, it appears of a dull brown, that ariſes from a mixture of the red and green.*

Probability that Mr. Chenevix was miſled to infer that palladium is a compound, from its being found in platina.

The characters of palladium that have been enumerated, undoubtedly belong to none of the ſimple ſubſtances that we are acquainted with; and no experiment that I have made, has tended to confirm the ſuſpicion of its being a compound, conſiſting of any known ingredients. The experiments above related, ſhow evidently, that the ore of platina contains a very ſmall quantity of palladium; and it is not unlikely that this may have been a conſtituent part of ſome of the compounds obtained by Mr. Chenevix, and may have miſled him, by ſome properties which he would conſequently obſerve, into the ſuppoſition that he had formed palladium.

All attempts of the author to compare it were unſucceſsful.

It is not, however, without having repeatedly endeavoured to imitate his experiments, that I have ventured to diſſent from ſuch authority. I made many attempts to unite pure platina with mercury, by ſolution, and by amalgamation; but without ſucceſs, in any one inſtance.

From a ſolution of platina, carefully neutralized, as Mr. Chenevix directs, with red oxide of mercury, and mixed with a ſolution of green ſulphate of iron, I indeed obtained ſuch a precipitate of metallic flakes as he deſcribes; but, upon examination of theſe flakes, they yielded mercury by diſtillation; and the remainder conſiſted of platina combined with a portion of iron, but had not any properties which I could ſuppoſe owing to the preſence of palladium.

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* The change of colour above deſcribed, though certainly uncommon, is nevertheleſs not peculiar to the ſalts of palladium, but may be ſeen alſo in ſome kinds of tourmalin. Among thoſe which come to us from Ceylon, ſome are tranſparent; and one variety is of a deep red in the direction of its axis, but of a yellowiſh green when viewed tranſverſely. There is alſo a correſponding, but oppoſite contraſt of colours, that has been obſerved by Muller, and deſcribed by Bergmann, in ſome of the Tyroleſe tourmalins. The general aſpect of theſe ſtones is black, and apparently opaque.—Some, however, of which the fracture is vitreous, are found to tranſmit a yellowiſh-red light when viewed tranſverſely, but in the direction of their axis the colour is a dull bottle green.

In each of theſe tourmalins, as well as in the ſalts of palladium, the colour in the direction of the axis is at leaſt ten times more intenſe than in the tranſverſe direction. A thin lamina, cut from the end of a Tyroleſe tourmalin for this purpoſe, tranſmitted no viſible light, till it was reduced to $\frac{1}{60}$ of an inch in thickneſs; and, when leſs than $\frac{1}{100}$ of an inch, it was not more tranſparent than another portion of the ſame cryſtal ſeen tranſverſely, $\frac{1}{10}$ of an inch in thickneſs.

Other facts and obſervations to ſhow that palladium is a ſimple metal.

Upon comparing the ſpecific gravity of this ſubſtance, which was ſaid to be, at moſt, 11,8, with that of mercury or of platina, I was always ſtrongly inclined to doubt the poſſibility of its being compoſed of theſe metals. I could recollect no one inſtance, in which the ſpecific gravity of any compound is leſs than that of its lighteſt ingredient, and could not, without careful examination, admit the ſuppoſition, that mercury could be rendered lighter by intimate union with platina. It now appears fully confirmed that this perſuaſion, ariſing from uniform experience, was well founded; for, if we conſider the difficulty of producing even an imperfect imitation of palladium, the failure of all attempts to reſolve it into any known metals, the facility of ſeparating it from any mixed ſolution of thoſe which it has been ſuppoſed to contain, as well as the number and diſtinctneſs of its characteriſtic properties, I think we muſt claſs it with thoſe bodies which we have moſt reaſon to conſider as ſimple metals.

XI.

Letter from Mr. WM. WILSON, *exhibiting the Electricity of Metals, without the Help of any condenſing Inſtrument.*

To Mr. NICHOLSON.

SIR,

London, Dec. 22, 1804.

Whether the electricity from metals alone be the effect of the contact or the ſeparation,

WHEN I ſet about making the compound electrical condenſer deſcribed in my laſt letter to you, I intended to repeat the experiments of Cavallo relating to the electricity obtained by the contact of metals related in the third volume of the fourth edition of his Treatiſe of Electricity; but before the inſtrument was finiſhed, I was induced (by ſome experiments I had made relative to the cauſe of excitation of electricity) to ſuppoſe that it is not the contact of the metals that is the cauſe of the appearance of electrical ſigns, but the ſeparation of the metals from contact. And this ſuppoſition was very much ſtrengthened, when, upon examination, I found that all who have made experiments on this ſubject have ſeparated the metals from contact before they examined them as to their electricity.

—determinable by varying the ſurfaces.

If the contact of the two metals be the cauſe of the electrical ſigns, the whole effect that one metal can have on another will be communicated at the time of contact, however few the points are that form the contact, becauſe both the metals being conductors of electricity, if one poſſeſſed a greater proportion of it than the other, a part will be communicated to the other at the time of contact, to form an equilibrium, and this will be done as well by a few points of contact as by a great many. But if it is the ſeparating them from contact, that is the cauſe of the electrical ſign, the more extenſive the contact is the more powerful will the ſigns be when the metals are ſeparated.

Sieve of one metal and filings of another.

To put this to the teſt of experiment, I pierced a piece of thin ſheet copper full of ſmall holes, juſt big enough to permit to paſs through them two or three particles of filings of another metal at a time, ſo that almoſt every particle muſt be in contact with the copper before it can paſs through, and conſequently the ſurface of contact be very great with a comparatively ſmall quantity of metal.

I ſifted through this copper ſieve ſome filings of zinc into a tin plate laid on the cap of a gold leaf electrometer, and the gold leaves diverged near an inch with poſitive electricity, when about half an ounce of filings had been ſifted into it. Encouraged by this ſtriking reſult, I procured ſieves and filings of different metals. The reſults of the trials with them are contained in the following table; where P ſtands for poſitive electricity, N for negative, and when it was not ſtrong enough to effect the electrometer, that is denoted by O.

Reſults of experiments.

Filings of	Sifted through	Effect.
Copper	Copper	O.
	Tin	N. ſtrong
	Silver	O.
	Zinc	P. ſtrong
	Lead	N.
Zinc	Copper	P. ſtrong
	Tin	O.
	Silver	P. ſtrong
	Zinc	P. exceeding weak
	Lead	O.

Reſults of experiments.

Filings of	Sifted through	Effect.
Tin	Copper	P. rather ſtrong
	Tin	N. very weak
	Silver	P. ſtrong
	Zinc	O.
	Lead	N. weak
Silver	Copper	O.
	Tin	N. weak
	Silver	O.
	Zinc	N.
Steel	Copper	N. very ſtrong
	Tin	N. ditto
	Silver	N. ditto
	Zinc	N. ditto
	Lead	N. ditto
Lead	Copper	P. very ſtrong
	Tin	O.
	Silver	P. very ſtrong
	Zinc	P. not quite ſo ſtrong as with ſilver
	Lead	O.
Braſs	Copper	P. very ſtrong
	Tin	N. ſtrong
	Silver	P. ſtrong
	Zinc	O.
	Lead	N.
Biſmuth	Copper	P. exceeding ſtrong
	Tin	O.
	Silver	P. exceeding ſtrong
	Zinc	P. ſtrong
	Lead	N.
Antimony	Copper	O.
	Tin	N. very ſtrong
	Silver	P. ſtrong
	Zinc	N. ſtrong
	Lead	N. very ſtrong
Nikel	Copper	P. exceeding ſtrong
	Tin	O.
	Silver	P. ſtrong
	Zinc	O.
	Lead	P.

In all the above experiments there was a large ſurface of contact, and the electrometer only was uſed; but in thoſe made by Cavallo and others who had a very ſmall ſurface of contact, electrical ſigns could not be made to appear without the help of doublers, multipliers, &c. of electricity. I therefore think there can be no doubt about the ſeparating the metals from contact being the cauſe of their appearing.

If you think the above worthy of a place in your Phlioſophical Journal, you will very much oblige me by inſerting it therein. I am your obedient humble ſervant,

WM. WILSON.

XII.

Analytical Experiments and Obſervations on Lac. By CHARLES HATCHETT, *Eſq. Partly abridged, but chiefly extracted from the Philoſophical Tranſactions for* 1804, *by* W. N.

Uſes of lac peculiar to India.

IT is uncertain at what time the uſeful ſubſtance, called Lac, firſt became known in Europe. It has long been uſed by the Indians for purpoſes different from thoſe to which it is applied in Europe. In particular as Mr. Wilkins informed the author, they diſſolve ſhell lac in water by the addition of a little borax, and by adding lamp black or ivory black, they obtain an ink, not eaſily affected by water, when dry.

Aqueous ſolution by borax affords an ink.

Natural hiſtory of lac.

The natural hiſtory of lac, which is the nidus of the coccus or chermes lacca, has been detailed by Mr. Kerr, Mr. Saunders, and Dr. Roxburgh, in the Philoſophical Tranſactions for 1781, 1789, and 1791. It is diſtinguiſhed into four kinds, of which, however, only three are commonly known in commerce, *viz.* ſtick lac, ſeed lac, and ſhell lac; the difference of theſe, with that of the fourth, called lump lac, is as follows.

Four kinds.

1. Stick lac, is the ſubſtance or comb in its natural ſtate, incruſting ſmall branches or twigs. 1. Stick lac.

2. Seed lac, is ſaid to be only the above, which has been ſeparated from the twigs, and reduced into ſmall fragments; but I ſuſpect it to have undergone ſome other proceſs, as I have found the beſt ſpecimens to be very conſiderably deprived of the colouring matter.* 2. Seed lac.

3. Lump

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* Mr. Wilkins informs me that the crude lac, as it is taken from the branches and twigs of the trees, is uſually deprived of its colour-

ing

3. Lump lac.

3. Lump lac, is formed from ſeed lac, liquefied by fire, and formed into cakes. And,

4. Shell lac.

4. Shell lac, according to Mr. Kerr and Mr. Saunders, is prepared from the cells, liquefied, ſtrained, and formed into thin tranſparent laminæ, in the following manner.

How made from the cells.

"Separate the cells from the branches; break them into ſmall "pieces; throw them into a tub of water, for one day; waſh "off the red water; dry the cells, and with them fill a cylindri-"cal tube of cotton cloth, two feet long, and one or two inches "in diameter; tie both ends, and turn the bag above a charcoal "fire; as the lac liquefies, twiſt the bag, and, when a ſufficient "quantity has tranſuded the pores of the cloth, lay it upon a "ſmooth junk of the plantain tree, and with a ſtrip of the plan-"tain leaf draw it into a thin lamella; take it off while flexible, "for in a minute it will be hard and brittle." †

The degree of preſſure on the plantain tree, regulates (according to Mr. Saunders) the thickneſs of the ſhell; and the quality of the bag determines its fineneſs and tranſparency.

Lac is moſtly had from Aſſam.

Aſſam furniſhes the greateſt quantity of the whole of the lac now in uſe. ‡

Beſt quality.

Mr. Kerr (ſpeaking of ſtick lac) obſerves, that the beſt lac is of a deep red colour; for, if it is pale and pierced at the top, the value is diminiſhed, becauſe the inſects have left their cells, and conſequently theſe can be of no uſe as a dye or colour, but probably may be better for varniſhes.

The ſeed lac which I have examined, contained but little of the colouring matter, and appeared (as I have already obſerved) to have undergone ſome proceſs of purification; but, of all the varieties, ſhell lac contains the leaſt of the tinging ſubſtance, as may well be expected, when the mode of preparing it is conſidered.

Chemiſts have paid little attention to lac.

It is remarkable, that although lac has been known, and imported into Europe, during ſo long a time that the date cannot now be aſcertained, yet it has but little attracted the attention of chemiſts.

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ing matter by boiling, having been previouſly reduced, by pounding into ſmall fragments. In Bengal, the ſilk dyers are the people who thus produce what we call the ſeed lac, which they do for the ſake of the colour.

† Phil. Trans. 1781, p. 378.

‡ Phil. Tranſ. 1789, p. 109.

The firſt chemiſt of eminence who mentions it, and the only one who has ſubjected it to any thing like a regular examination, is the younger Geoffroy, whoſe Paper is publiſhed in the *Mém. de l'Acad. de Paris* for the year 1714.* In this Paper, Mr. Geoffroy ſeems to have been chiefly induced to examine it on account of its tinging ſubſtance; but he nevertheleſs has not neglected the ſubſtance which conſtitutes the cells. This he conſiders to be a ſort of wax, very diſtinct from the nature of gum or reſin. But it is to be obſerved, that he formed this opinion, not ſo much upon the reſults of chemical experiments, as upon the cellular conſtruction obſerved in the ſtick lac, which, as he juſtly remarks, demonſtrates it to be formed by inſects, after the manner that the honeycomb is formed by bees; and that it is not therefore, as ſome have ſuppoſed, a gum or reſin, which has exuded from vegetables ſimply punctured by inſects.†

Experiments of Geoffroy the younger;

Geoffroy and Lemery obtained from lac, by diſtillation, ſome acid liquor, and a butyraceous ſubſtance. Moreover, Geoffroy obſerves, that when ſtick lac was thus treated, ſome ammonia was alſo obtained, but not when ſeed lac was employed.

—and Lemery.

He alſo mentions another ſort of lac, brought from Madagaſcar, and called by the natives *Lit-in-bitſic*. This ſubſtance, he ſays, is ſcarcely to be diſtinguiſhed from bees-wax, which it much reſembles in colour and odour; and that it is produced by a grayiſh inſect, much larger than the chermes lacca. It is evident however, from Geoffroy's deſcription, that this ſubſtance is very different from the common lac; and there can be little doubt, but that it is the ſame as that which was, a few years ago, examined by Dr. Pearſon, under the name of white lac, a ſubſtance reſembling the Pé-la of the Chineſe.‡

Another lac which appears to have been the Pe-la of China.

Geoffroy (as I have ſtated) conſidered lac as a ſort of wax; and ſince his time it has ſcarcely, if at all, been ſubjected to chemical

Notions of chemiſts concerning lac;

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* Obſervations ſur la Gomme Lacque, et ſur les autres Matières animales qui fourniſſent la Teinture de Pourpre. Par. M. Geoffroy le jeune. *Mém. de l'Acad.* 1714. p 121.

† Mr. Kerr obſerves, that as a red ſubſtance is obtained by inciſion from the plaſo tree, very analogous to lac, it is probable, that the inſects have little trouble in animalizing the ſap of theſe trees, in the formation of their cells. Phil. Trans. 1781, p. 377.

‡ Phil. Trans. 1794, p. 383.

chemical examination; it is not therefore ſurpriſing, that the opinions of chemiſts concerning it have been various. Chaptal adopts the opinion of Geoffroy, and calls it a kind of wax;* but Gren † and Fourcroy ‡ regard it as a true reſin.

§ I.

Chemical Experiments on Lac. Its Habitudes with Solvents.

Water extracts the colour from lac.

1. Water digeſted upon powdered ſtick lac becomes of a deep crimſon colour, and takes up about 10 per cent. Seed lac gives out no more than 2½ or 3 per cent. and ſhell lac only ½ per cent. by ſimilar treatment.

Alcohol diſſolves the reſin, which is a large part.

2. Alcohol diſſolves much of the lac, taking up reſin in a clear ſolution if cold; but if heated, the ſolution is turbid and ſcarcely to be rendered tranſparent, either by repoſe or filtration.

The reſin is obtained either by evaporating the ſolution or by pouring it into water acidulated with muriatic or acetic acid. The reſin which is thus precipitated may be ſeparated by the filter. It amounts to 67 or 68 per cent. in ſtick lac, and to 88 in the ſeed lac. Shell lac contains 81 per cent. beſides 10 per cent. which is defended from the ſpirit, and cannot be ſeparated but by ſubſequent operation. The reſin is leſs brittle than other reſin.

Vegetable gluten.

When the ſhell lac was in ſmall fragments only, theſe after the firſt ſeparation of the reſin, retained their figure and were more bulky, porous, and elaſtic. The elaſticity was deſtroyed by boiling water, and the matter itſelf proved to be of the nature of vegetable gluten.

Ether affects lac very little.

3. Sulphuric ether acts leſs powerfully on lac than alcohol does. It conſiderably ſoftens the lac; but produces little other effect.

Sulphuric acid converts it into coal.

4. Concentrated ſulphuric acid firſt acts on the colouring matter; but after a ſhort digeſtion on the ſand bath, the fluid acts on the whole maſs, and becomes firſt reddiſh brown, and afterwards black, ſulphureous acid gas being evolved, and the chief part of the lac being at length ſeparated in a ſtate reſembling coal.

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[*] Chaptal's Elements; Engliſh edition. Vol. III. p. 387.

[†] Principles of modern Chemiſtry. Vol. I. p. 388.

[‡] *Syſtême des Connoiſſances chimiques.* Tome V. p. 624.

Nitric

5. Nitric acid applied to lac emits nitrous gas at firſt, and cauſes the lac to ſwell much at the ſame time that it converts it into a deep opake yellow brittle ſubſtance, which by a ſufficiency of nitric acid, and continuance of the digeſtion for 48 hours, is diſſolved. The ſolution however is turbid, and when poured into a large quantity of diſtilled water, depoſits ſome yellowiſh flocculi, which, being collected, are found to be a ſort of wax.

Nitric acid by management and time diſſolves it.

The ſolution is turbid and by dilute depoſits a wax.

The filtrated liquor is of a bright golden yellow; and, when ſaturated by ammonia, changes to orange colour, but does not yield any precipitate, nor any traces of oxalic or malic acid.

The filtered ſolution contains a yellow matter.

This yellow nitric ſolution is converted, by evaporation, into a deep yellow ſubſtance, which burns like reſin, but is ſoluble in boiling water.

Its properties, &c.

The alkalis and lime, being added to this aqueous ſolution, do not produce any precipitate, but the yellow colour is very conſiderably deepened; and, by evaporation, an orange-coloured ſubſtance is obtained, which is ſtill eaſily ſoluble in water, and conſiſts of the deep yellow ſubſtance above-mentioned, combined with the alkali or lime.

6. Muriatic acid diſſolves the colouring matter and gluten of lac; but its action on theſe is feeble, unleſs the reſin has been previouſly ſeparated.

6. Muriatic acid diſſolves the colouring matter and gluten.

7. Acetous acid, in its effects, much reſembles muriatic acid.

7. Acetous acid.

8. Stick lac, ſeed lac, and ſhell lac, are partially diſſolved by acetic acid; and, if this be heated, a conſiderable portion is taken up.

8. Acetic acid acts upon the whole except the wax.

The diſſolved part conſiſts of the colouring extract, of reſin, and of gluten; the wax being the only ingredient which is inſoluble in this menſtruum; but a portion of the former ſubſtances, being enveloped by the wax, are protected from the action of the acetic acid.

The acetic ſolution of lac becomes turbid when cold, and depoſits part of the reſin; a portion however remains in ſolution, and may be precipitated by water; after which, the liquor retains ſome gluten and colouring extract, which may be precipitated by ſaturating the acid with an alkali, and by ſubſequent boiling.

For the reaſons above ſtated, it would be difficult to effect a complete ſolution of lac by means of acetic acid; but this may nevertheleſs be advantageouſly employed in analytical operations, when alternately uſed with alcohol.

Boracic acid ſcarcely acts on lac.

9. A ſaturated ſolution of boracic acid in water, diſſolves the colouring extract; but, as the effect does not ſurpaſs that of water alone, we may conclude that lac is little, if at all, acted upon by boracic acid.

Borax renders it ſoluble in water.

10. It has been already ſtated, that ſub-borate of ſoda or borax has a powerful effect on lac, ſo as to render it ſoluble in water; and, as the preceding experiments prove that boracic acid alone ſcarcely acts upon lac, there is every reaſon to believe, that the exceſs of ſoda preſent in borax is the active ſubſtance; and this concluſion will be confirmed, by the reſults of ſubſequent experiments made with the alkalis.

Proportion, &c.

In order to render lac (eſpecially ſhell lac) ſoluble in water, about $\frac{1}{5}$ of borax is neceſſary; and this may be previouſly diſſolved in the water, or may be mixed and added together with the lac.

The beſt proportion of water to that of lac is 18 or 20 to 1. So that 20 grs. of borax, and four ounces of water, are, upon an average, requiſite to diſſolve 100 grs. of ſhell lac; but more water may be occaſionally added, to ſupply the loſs cauſed by evaporation during the digeſtion, which ſhould be made nearly in a boiling heat.

This ſolution of ſhell lac is turbid, and of a reddiſh-brown colour; when conſiderably diluted with water and agitated, a weak lather is formed; it is decompoſed by acids, and the lac is precipitated in yellow flocculi, which do not apparently differ from the lac originally employed.

The ſolution is a ſoap of difficult ſolubility.

The general properties of the ſolution ſhew, that it is a ſaponaceous compound, which, being uſed as a varniſh or vehicle for colours, becomes (when dry) difficultly ſoluble in water, although this was the liquid employed to form the ſolution.

A white thick ſcum or cream collects on the ſurface of this liquid, after it has been ſuffered to remain tranquil for ſome time, and is found to be produced by a ſort of wax, which I ſhall more particularly notice when the analyſes of the varieties of lac are deſcribed; but, in the preſent caſe, this wax appeared in ſome degree to be converted into an almoſt inſoluble ſoap by the alkali of the borax, and may be regarded as the principal cauſe of the turbidneſs of the ſolution.

11. The

Soda and its carbonate diſſolve lac.

11. The lixivia of pure ſoda and of carbonate of ſoda completely diſſolve the different kinds of lac; and theſe ſolutions exactly reſemble thoſe formed by means of borax, excepting that they are deeper coloured.

Rather leſs than $\frac{1}{5}$ of carbonate of ſoda is required to diſſolve ſhell lac; and this ſolution, when dried, is ſooner affected by damp or water than the ſolution prepared by borax.

Alſo potaſh,

12. Lixivium of pure or cauſtic potaſh ſpeedily diſſolves the varieties of lac, and forms ſaponaceous ſolutions, ſimilar to that in which borax was employed, excluſive of the colour, which is deeper, and more approaching to purple.

and its carbonate.

Lixivium of carbonate of potaſh extracts a great part of the colouring matter, but does not form ſo complete a ſolution of the entire ſubſtance of lac, as when pure potaſh is employed.

The above alkaline ſolutions, by repoſe, afford the waxen ſoap which has been mentioned; and acids, being added to theſe ſolutions, and to that formed by borax, precipitate the lac in a flocculent ſtate, and of a yellow or buff colour, which precipitate, when melted, becomes ſimilar to the lac originally employed. If however an alkaline ſolution of ſhell lac (prepared, for inſtance, with ſoda) be gradually dropped into a ſufficient quantity of muriatic acid diluted with an equal portion of water, and nearly heated to the boiling point, and if after boiling the whole for about one hour the coagulum be ſeparated, and the clear liquor be carefully ſaturated with ſoda, and again made to boil, a ſmall quantity of a flocculent precipitate is obtained, which was found to be analogous to precipitated vegetable gluten, combined with ſome of the colouring extract.

Ammonia acts partially.

13. Pure ammonia, and carbonate of ammonia, readily act upon the colouring matter of lac, but do not completely diſſolve the entire ſubſtance.

§ II.

Analytical Experiments on Stick, Seed, and Shell Lac.

Products of lac by deſtructive diſtillation. Stick lac.

Lac, placed on a red hot iron, contracts, melts, ſmokes much, and leaves a ſpongy coal. Slow diſtillation carried to the red heat, gave from ſtick lac, 1. Water ſlightly acid, 10;

2. Thick brown butyraceous oil, 59; 3. Spongy coal, 13$\frac{1}{2}$; 4. A ſmall portion of carbonate of ammonia, with carbonic acid, carbonated hidrogen, and hidrogen gas, by eſtimate 17$\frac{1}{2}$.

Seed lac.

Seed lac, by the ſame proceſs, gave, 1. Acidulated water, 6; 2. Thick oil, 61; 3. Spongy coal, 7; 4. Mixed gas as before, but without ammonia, 26.

Shell lac.

Shell lac gave, 1. Acidulated water, 6; 2. Oil, 65; 3. Spongy coal, 7$\frac{1}{2}$; 4. Mixed gas, 21$\frac{1}{2}$.

The coal of this laſt gave one grain of aſhes after combuſtion, which contained a muriate, probably of ſoda. There was alſo a little iron, and ſome particles of ſand, probably extraneous.

Analyſis of Stick Lack.

Powdered ſtick lac gave a red colouring extract to water.

A. 200 grains of ſtick lac, picked and reduced to powder, were digeſted in a pint and a half of boiling diſtilled water during 12 hours. The liquor was tranſparent, and of a beautiful deep red; this was decanted into another veſſel; and the operation was repeated, with freſh portions of water, until it ceaſed to be tinged; the lac then appeared of a pale yellowiſh-brown colour.

The whole of the aqueous ſolution being evaporated, left a deep red ſubſtance, which poſſeſſed the general properties of vegetable extract, and weighed 18 grains.

Alcohol then took up much reſin.

B. The dried lac was digeſted for 48 hours, without heat, in eighteen ounces of alcohol; and the clear tincture being cautiouſly decanted different portions of alcohol were added, and the digeſtion was repeated, until the alcohol ceaſed to produce any effect.

The whole of the ſolutions in alcohol were then poured into diſtilled water, which was heated, and an attempt was made to ſeparate the precipitated ſubſtance by filtration; but, as this did not ſucceed, on account of the filter ſpeedily becoming clogged, the whole was ſubjected to gentle diſtillation; by which, a browniſh-yellow reſin was obtained, amounting in weight to 136 grains.

C. The remainder of the lac was again digeſted in boiling diſtilled water; by which, 2 grains of the colouring extract were obtained.

Diluted muriatic acid took gluten from the remainder.

D. The reſiduum was then digeſted with one ounce of muriatic acid diluted with two ounces of water, which, by boiling, became of a bright pale red, but changed to purple, when ſaturated with a ſolution of carbonate of potaſh.

A flocculent precipitate was thus obtained, which poſſeſſed the characters of precipitated vegetable gluten combined with ſome of the colouring extract; this, when completely dried, weighed 11 grains.

The reſidue was wax and foreign matter.

E. There now remained 25 grains, which evidently conſiſted of a ſort of wax, mixed with ſmall parts of twigs and other extraneous ſubſtances.

A part of the wax was ſeparated by heat and preſſure in a piece of linen; and another portion was ſeparated by digeſtion in olive oil, which aſſumed the conſiſtency of an unguent.

The reſiduum was then boiled with lixivium of potaſh, and became tinged with purple, in conſequence of ſome of the colouring extract which had not been diſſolved by the preceding operations.

The undiſſolved part, now conſiſting only of the extraneous vegetable and other ſubſtances, weighed 13 grains; ſo that the wax, with a ſmall portion of the colouring extract, may be eſtimated at 12 grains.

Component parts of ſtick lac.

By the above proceſs, 200 grains of ſtick lac afforded,

			Grs.
A. C.	Colouring extract	18 2	20
B.	Reſin		136
D.	Vegetable gluten		11
E.	Wax, with a little colouring extract, about		12
	Extraneous ſubſtances		13
			192.

Analyſis of Seed Lac.

Component parts of ſeed lac.

200 grains of very pure ſeed lac were ſubjected to operations very ſimilar to thoſe which have been deſcribed, and afforded,

	Grs.
Colouring extract	5
Reſin	177
Vegetable gluten	4
Wax	9
	195.

Analyſis of Shell Lac.

Shell lac boiled in water gave little extract.

A. 500 grains of this ſubſtance were firſt treated with boiling diſtilled water, as above-mentioned, and yielded of extract only 2.50 grains.

Reſidue gave much reſin to alcohol.

B. The 497.50 grains which remained, were then digeſted with different portions of cold alcohol, until this ceaſed to produce any effect; the reſin which was thus ſeparated, amounted to 403.50 grains.

C. As the ſhell lac had not been reduced into powder, but only into ſmall fragments, theſe were become white and elaſtic, and, when dry, were brittle, and of a pale brown colour; the whole then weighed 94 grains.

Muriatic acid then took up gluten.

D. Theſe 94 grains were digeſted in diluted muriatic acid; and the acid, being afterwards ſaturated with ſolution of carbonate of potaſh, afforded a flocculent precipitate, (reſembling that obtained from ſolutions of vegetable gluten,) which, when dry, weighed 5 grains.

Acetic acid, by long digeſtion, took up reſin and wax; the latter was thrown down by alcohol;

E. Alcohol acted but feebly on the reſiduum; it was therefore put into a matraſs, with three ounces of acetic acid, and was ſuffered to digeſt without heat during ſix days, the veſſel being at times gently ſhaken; the acid thus aſſumed a pale brown colour, and was very turbid. The whole was then added to half a pint of alcohol, and was digeſted in a ſand-bath; by which a browniſh tincture was formed, and at the ſame time a quantity of a whitiſh flocculent ſubſtance was depoſited, which, being collected, well waſhed with alcohol on a filter, and dried, weighed 20 grains.

This ſubſtance was white, light, and flaky, and, when rubbed by the nail, it became gloſſy, like wax; it alſo eaſily melted, was abſorbed by heated paper, and, when placed on a coal or hot iron, emitted a ſmoke, the odour of which very much reſembled that of wax, or rather ſpermaceti.

and then the reſin by water.

F. The ſolution formed by acetic acid and alcohol, being filtrated, was poured into diſtilled water, which immediately became milky; and, being heated, the greater part of the reſin which had been diſſolved aſſumed a curdy form, and was partly ſeparated by a filter, and partly by diſtilling off the liquor; this portion of reſin amounted to 51 grains.

Carbonate of potaſh threw down ſome reſin,

G. The filtrated liquor, from which this reſin had been ſeparated, was ſaturated with a ſolution of carbonate of potaſh; and, being heated, a ſecond precipitate of gluten was obtained, which, when well dried, weighed 9 grains.

Component parts of ſhell lac.

The 500 grains of ſhell lac thus yielded,

		Grs.
A.	Extract - - -	2.50
B. C.	Reſin - - - -	454.50
D. G.	Vegetable gluten - -	14.
E.	Wax - - - -	20.
		491.

Advantages of the proceſs laſt uſed.

The mode of analyſis adopted for the ſhell lac, muſt undoubtedly appear leſs ſimple than that which was employed for ſeed and ſtick lac: but, upon the whole, it was attended with advantages; for the ſhell lac being in ſmall fragments, and not in the ſtate of a powder, conſiderably facilitated the decantation of the ſolution in alcohol from the reſiduum; and although, in this laſt, a portion of the reſin was protected from the action of the alcohol, by being enveloped in the gluten and wax, yet, by the aſſiſtance of acetic acid, the remainder of the reſin, as well as the whole of the gluten, were diſſolved; the wax was obtained in a pure ſtate; and a ſeparation of the reſin from the gluten was afterwards eaſily effected, by the method which has been deſcribed. As therefore acetic acid is capable of diſſolving reſin, gluten, and many other of the vegetable principles, it certainly may be regarded as a very uſeful ſolvent, in the analyſis of bodies appertaining to the vegetable kingdom.

From the reſults of the preceding analyſes it appears, that the different kinds of lac conſiſt of four ſubſtances, namely, extract, reſin, gluten, and wax, the ſeparate properties of which ſhall now be more fully conſidered.

(The Concluſion in our next.)

Galvanic

XIII.

Galvanic Illuſtrations and Remarks. *By* C. WILKINSON, *Eſq.*

To Mr. NICHOLSON.

DEAR SIR, *Glaſgow, Dec.* 9, 1804.

On the galvanic charge, &c.

HAVING, during my galvanic tour, been occaſionally informed that the attempt I have made in my Elements of Galvaniſm, to explain the laws of galvaniſm on the more perfect and imperfect conductors, is not ſo clear as it was my wiſh to have rendered it; I ſhall therefore deem myſelf much obliged by the inſertion of the following obſervations in your valuable Journal.

The power of a galvanic apparatus, formed of plates not leſs than 2¼ inches in diameter, on animal ſubſtances, is directly as the number of plates; on the more perfect conductors, as metals, the power is as you have at large ſtated in your laſt Journal.

Let us ſuppoſe three galvanic plates: *Fig.* 1, *(Plate II.)* of four inches; *Fig.* 2, of ſix inches; and *Fig.* 3, of eight inches: An arrangement of theſe in the form of a galvanic trough, produces equal effects on animal ſubſtances, provided the number be equal.

Animal ſubſtance is very little ſuperior to water in its conducting power; and the experiments of Mr. Cavendiſh have proved, that the conducting powers of iron are a million times greater than the conducting powers of water.

A metal brought into contact with another metal diſpoſed to part with a quantity of its compound electricity, will divide the quantity thus diſengaged, becauſe its capacity for receiving it is equal to that of the metal under the chemical action.

Animal ſubſtance brought into contact with a metal ſimilarly diſpoſed, will only receive a portion adequate to its capacity; and as this capacity is in the ratio of its conducting powers, it will be nearly one million times inferior in capacity to that of a metallic ſubſtance; and will, therefore, abſtract only ſo ſmall a proportionate part of electricity to render its intenſity equal to that exiſting in the galvanic plate.

By electrical intenſity I underſtand the quantum of electricity multiplied by the reſiſting powers of the ſubſtance to admit it. Let us ſuppoſe the plates 1, 2, and 3, diſpoſed to give out electricity on every aſſignable point of their reſpective ſurfaces. A finger applied at No. 1, *Fig*. 1, will abſtract a certain portion of electricity from the points of metal directly in contact; a certain portion of electricity will immediately be determined from the ſquares 2, 3, 4, 5, 6, &c. from every correſponding ſquare on the ſurface of the plate, by that motion which Volta has well termed the moto-electrical power: there is a phyſical reſiſtance in the tranſmiſſion from theſe reſpective ſquares to the firſt touched by the finger, and the more remote the greater the reſiſtance: at whatever points this phyſical reſiſtance ſhall be equal to the capacity of the ſubſtance employed, there all further tranſmiſſion muſt ceaſe: this reſiſtance in plates of 2¼ inches in the ſquare, appears, from experiment, fully adequate to the conſtant ſupply of ſo imperfect a conductor as animal ſubſtances, and therefore any increaſe of ſize in the ſurface of the plate, will produce no additional effects.

When a certain quantity is thus abſtracted from the pair of plates at the extremes of the battery, in direct contact with the fingers, theſe plates and the interpoſed fluid in the cells, only act as conductors to the next pair, and ſo on; as the velocity of diſcharges of each ſucceſſive pair is ſo infinitely greater than the time required for the chemical action, is the reaſon why the effects we experience are increaſed in proportion to the numbers: if we were to ſuppoſe that the time required for the ſucceſſive diſcharge of 50 pair of plates, ſhould be preciſely equal to the time required for the chemical action, we ſhould derive no increaſe of power by an increaſe of number, which is contrary to experience.

I have a battery formed of 600 plates, each plate only expoſing half a ſquare inch in ſurface: To experience equal effects from this arrangement, I am obliged to pauſe three or four minutes between each contact; an interval of time no ways requiſite in larger plates.

When ſuch ſuperior conductors as metals are employed, the phyſical reſiſtance of tranſmiſſion from one part of the plate to another, bearing no proportion with the capacity of the metal, every aſſignable point of the galvanic plate is efficient at the ſame time, therefore the intenſity will be as the ſurface, and the powers as the ſquares of the intenſities.

I am, Dear Sir,
Your's, reſpectfully,
C. WILKINSON.

No. 19, *Soho Square.*

XIV.

On the Devitrification of Glaſs and the Phenomena which happen during its Cryſtallization. By DARTIGUES.*

Glaſs is not a cryſtallization.

SOME philoſophers have conſidered glaſs as a chryſtallization; and this opinion appears natural on conſidering the tranſparence of glaſs or of cryſtal, from the latter of which the term for deſignating the regular and ſpontaneous arrangement of the particles of bodies has been taken. But on cloſer reflection, this notion is found to be erroneous. In fact glaſs never affects a cryſtalline figure, either in its ſurface or its fracture; it never preſents chryſtals of its own proper ſubſtance, ſuch as are ſeen in metals ſlowly congealed; and whenever cryſtals are formed in the maſs of glaſs, they are foreign to the part which ſtill continues vitrified; they may be conſidered as a ſtep the reverſe from that of vitrification, as I ſhall ſhow in the courſe of the preſent paper.

Vitreous fuſion diſtinct from that of ſalts.

On the firſt explanation of vitreous fuſion, I muſt diſtinguiſh and ſeparate it from the fuſion of certain other bodies which flow without addition in the heat of our furnaces, ſuch as borax, the phoſphoric acid and others. In theſe bodies the condenſed heat ſoftens and renders them fluid, and they more or leſs continue to exhibit, when cooled, the tranſparency and ſome other of the well known phyſical properties of glaſs.

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* Tranſlated from the Journal de Phyſique, LIX. 5.

The author of this memoir, who is proprietor of the glaſs-works and other eſtabliſhments of Voneche (Sambre-et Meuſe) has undertaken to draw up for the French National Inſtitute a treatiſe on the art of glaſs-making, to ſerve as a continuation to the arts and trades of the Academy. The firſt part is in readineſs to appear, many of the plates being ready. The ſecond part relates to the uſes of glaſs in the arts. And the third part conſiſts of detached memoirs upon the phyſical and chemical properties of glaſs. The preſent diſſertation is extracted from one of thoſe memoirs.—J. C. Delametherie.

It is a ſolution by heat.

But in the examination and deſcription of that fuſion to which vitreous compounds are ſubjected in order to fit them for the purpoſes of life, it is neceſſary to obſerve that the effect ariſes from two phenomena. It is not ſimply the reſult of accumulated heat, but it is jointly produced by the affinities of the ſubſtances which enter into the mixture. Theſe ſubſtances which tend to combine and mutually to penetrate each other, act according to the laws of their affinities as ſoon as ever the temperature is ſufficiently elevated. Thus it is that a mixture of ſeveral earths becomes fuſed at a temperature which could not have rendered any one of them fluid.

Nature of the compound; or glaſs.

The common vitrification is therefore the reſult of a combination effected at an elevated temperature between different and heterogenous ſubſtances; and the product is a compound perfectly homogenous more or leſs tranſparent, elaſtic, and breaking in a peculiar manner, from which the term vitreous fracture is derived. This body is a remarkably bad conductor of heat and of electricity; it is capable of becoming ſoft at a temperature inferior to that at which it was fuſed, ſo that it may be rendered paſty, ductile, &c.

Devitrification.

The phenomenon during which all theſe properties diſappear is what I have called devitrification; an expreſſion which may at firſt ſeem extraordinary, but which the facts will ſhew to be perfectly accurate.

It is the conſequence of cryſtallization.

Several philoſophers have before noticed this effect; ſome have even made obſervations upon it, and have noted various circumſtances ſcarcely connected with each other; but I am not aware that any one has yet publiſhed a ſeries of reſearches proper to explain the effects, ſo as to ſhew that it is one of the known properties of all natural bodies, being in fact nothing more than the product of a cryſtallization.

The porcelain of Reaumur;

Reaumur firſt obſerved that a glaſs, more eſpecially if it be compoſed of different earths, as bottle glaſs is in general, may be decompoſed, and loſe its tranſparency and other vitreous properties. Fully occupied in his reſearches upon porcelains, he was deſirous of applying this diſcovery to the fabrication of potteries, and attributed the phenomenon to the ſubſtances in which he had cemented his glaſs. The fact was denominated the cementation of glaſs, and the product was called Reaumur's porcelain: no incident could be more effectually calculated to retard the true developement of the fact than ſuch a denomination of this kind.

Repetitions of the proceſs by d'Antic and others.

The labours of Boſc d'Antic upon the ſame object were alſo directed to no other purpoſe than that of obtaining a good pottery by this means, and of aſcertaining what cements were the beſt calculated to give new properties to this body. Thus it was that by calling the proceſs by the name of cementation, which depended in no reſpect upon what was added as the cement, others were miſled, who were induced to follow the courſe of experiments already began. The natural conſequence was, that ſcience gained nothing reſpecting this proceſs ſince the time of Reaumur. Many have ſince attempted the cementation of glaſs without perceiving any new reſult in the product.

Obſervation of cryſtals in glaſs.

Several perſons have ſince admitted the property in glaſs of affording cryſtals; but theſe remarks being more eſpecially made by artiſts, placed by their ſituation at the head of glaſs-works, have not afforded the ſcientific conſequences which might have been deduced from them. The directors of a great eſtabliſhment have ſeldom time to dwell upon the contemplation of ſmall effects; they are obliged to attend to too many things at the ſame time. Theſe remarks, though curious in themſelves, remained without connection; and no one thought or ventured to publiſh that the cryſtallization of glaſs, and cementation by the proceſs of Reaumur are abſolutely one and the ſame thing.

Experiments of Sir James Hall on whinſtone, &c.

Sir James Hall,* in his valuable experiments upon whinſtone and lava, publiſhed in the 14th volume of the Bibliotheque Britannique, aſcertained the property of theſe ſtones to become fuſed into glaſs, and to return to the ſtony ſtate, according to the circumſtances.

He did not purſue this devitrification among artificial products.

He called this laſt fact a devitrification. He ſaw that it was the effect of a precipitation, and explained it in a manner that was highly ſatisfactory and true; but being too much occupied in deducing from this fact arguments in favour of the volcanic geology, he neglected purſuing the intereſting philoſophical conſequences to which the phenomenon pointed the way. This is the taſk I have undertaken, and in the preſent memoir on devitrification, I ſhall give the reſults of my firſt experiments. As my ſituation gives me the uſe of a fire of extreme violence and continued for years together, it may have been in my power to make obſervations not within the reach of every one. The facts I ſhall report do partly explain themſelves; they are the reſult of laws to which all bodies are ſubjected. All the merit of the obſervation conſiſts in having ſeen them in ſubſtances, and on occaſions where it was not known that theſe laws do take place.

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* Firſt in the Edinburgh Tranſactions, and afterwards in our Journal. N.

It is very remarkably ſeen in glaſs furnaces.

The bottom of the furnaces for fonding or fuſing the glaſs uſually preſents large cavities excavated by the action of the fire, and of the corroſive ſubſtances which often flow out of the melting pots. Theſe cavities are filled with a kind of glaſs called picadil, which is compoſed of aſhes which become vitrified, ſtones of the furnace which become fuſed, and more particularly of glaſs which falls from the pots. Care is taken to remove this at each ſont or making of glaſs. When the furnace is nearly worn out, the cavities having become large, cannot be entirely cleanſed, but ſome of the picadil remains. When the furnace is extinguiſhed, this picadil undergoes an extremely ſlow cooling, becauſe it is ſurrounded with maſonry of ſeveral cubic fathoms, which is itſelf penetrated with heat that has continued for upwards of a year. I have always remarked that it was in the glaſs of theſe bottoms of the furnace that I found cryſtallizations through the maſs of glaſs, the reſt of which was very tranſparent and pure. Theſe cryſtallizations, which were always conſiderably regular and numerous, excited my curioſity, as they had done that of other glaſs-makers before me. I collected many ſpecimens, taking care to chuſe thoſe which appeared the moſt curious, and preſented the moſt extrordinary characters.

Cryſtals in glaſs.

Examination of the facts.

Soon afterwards, from comparing the pieces themſelves, and the circumſtances under which they had been produced; and by the combinations of remarks, trials, and experiments to imitate theſe cryſtallizations at pleaſure, I at laſt ſucceeded in diſtinguiſhing various claſſes, all of which are produced by the nature of the different circumſtances which accompany the formation of the glaſs. I ſhall give a rapid outline of theſe, without entering into any remarks on the devitrification which almoſt conſtantly takes place in the ſcoria of forge furnaces.

This laſt fact muſt have preſented itſelf to every one, and will without difficulty be referred to the obſervations I ſhall make.

The more compounded the glaſs the more eaſily it loſes its vitreous nature.

The firſt remark which offers itſelf is, that the more compounded the nature of the glaſs, the more eaſily and readily it will become devitrified: but in the ſame manner as a ſolvent loaded with a great number of ſaline ſubſtances of different kinds, ſuffers them to cryſtallize more confuſedly, ſo likewiſe it is found that theſe glaſſes do not preſent the moſt regular cryſtallizations. A precipitation takes place through the whole of the maſs; each of the component parts obeys the laws of affinities; at the ſame time the tranſparency ſpeedily diſappears, and we no longer ſee a piece of glaſs, but a ſtone. Through this confuſed maſs it is nevertheleſs impoſſible not to obſerve the rudiments of cryſtals. Such is the manner in which bottle glaſs comports itſelf in its devitrific action. Theſe approach very much to the ſtate of mere earthy glaſſes, having very little ſalt in their compoſition.

Bottle glaſs.

Experiment in the ſmall way.

Every one has an opportunity of keeping a common bottle of black glaſs in a fire long continued, and capable of ſoftening its paſte; it ſpeedily changes colour, becomes grey, and aſſumes the appearance of ſtone ware. In this manner we form the pottery of Reaumur, but without any proceſs reſembling cryſtallization.

Particular examination of the changes in glaſs by ſlow cooling.

But inſtead of obſerving the phenomenon in ſo ſmall a maſs, if we examine the bottoms of the glaſs furnaces in which theſe bottles are fuſed, we find that the glaſs is abſolutely devitrified, and has aſſumed an appearance ſo completely ſtony, that the moſt experienced eye can ſcarcely diſtinguiſh the bricks of the furnace from the part which has been glaſs. It is only by following the courſe of the devitrification in pieces leſs advanced, that we ſucceed in diſtinguiſhing the glaſs in a granulated ſtone, which has rather the appearance of coarſe pottery, or a ſtrongly baked clay.

Inſtance of the effect taking place in a ſhort time.

It often happens that the ſlow cooling of an hour or two is ſufficient to produce the entire devitrification of bottle glaſs. I have pieces of eight centimetres (2½ inches) thick, which I obtained at the glaſs works of Mr. Saget, of La Gare. One of the pots was taken out of the furnace to be replaced; the glaſs remaining at the bottom of this pot was preſerved from cooling during the time the pot itſelf acquired the common temperature, and by this means the nature of the glaſs was entirely changed. It no longer exhibits the tranſparency, but is a maſs of cryſtals, compoſed of ſmall needles, converging towards common centers. It has no longer the appearance of glaſs.

This fact ſhews with what facility bottle glaſs is devitrified, and always without the leaſt appearance of cementation. The infinite variety of ſubſtances which make up the compoſition of bottle glaſs, greatly modifies the phenomena which take place during their devitrification, and muſt, no doubt, influence the form of the cryſtals; but my opportunities of obſerving this kind of glaſs have not been numerous.

Greeniſh glaſs of Alſatia is leſs compounded, and changes more ſlowly. Firſt change, a bluiſh colour.

Paſſing therefore to glaſſes leſs earthy and compoſed of a leſs number of ſubſtances, when I make the ſame examination of the bottoms of the furnaces of glaſs for windows called glaſſes of Alſatia, or half clear glaſſes, into which is put a greater portion of pure ſand and of alkali, I obſerve nearly the ſame phenomenon; but as they are leſs ſudden, they are more eaſy to diſtinguiſh and to ſeparate. At the firſt inſtant, and in the maſſes where the devitrification commences, we ſeem to obſerve a ſmall portion of blue colour diffuſed through a greeniſh liquid.

A very ſingular fact here preſents itſelf, which I ſhall here only point out, with the intention of more fully examining it hereafter. This greeniſh glaſs mixed with blue, appears, in fact, to become of an obſcure blue when conſidered by reflected light; but if placed ſo as to tranſmit the light, it always appears greeniſh; the blue being reflected and the green refracted, each ſingly.

Second change, dull white opacity.

By continuing to obſerve the devitrification of this glaſs, it is ſoon perceived that the blue precipitation is followed by another more abundant, which affords a dirty white, and is very diſtinct from the former. The colour of this glaſs becomes deeper and deeper, and at length it reſembles grey horn.

Third change, needled cryſtals in groupes.

In theſe different tranſitions the paſte of the glaſs appears conſtantly to remain, diſtinguiſhable in its poliſh, its fracture, and all its other properties, except its tranſparency. But in the midſt of this paſte reſembling horn, very diſtinct cryſtallizations are formed, conſiſting of nodules compoſed of ſmall needles, all converging towards the center. In this ſtate it is no longer glaſs, but a cryſtal poſſeſſing all the phyſical properties of mineral ſubſtances left to themſelves.

An exact analyſis made of a certain number of theſe cryſtals carefully detached from the maſs, might indicate their nature and throw light on their formation.

It often happens that theſe cryſtallized nodules are enveloped in a cruſt which ſeems foreign to their nature, and may be compared to the cruſt which envelopes flints in the middle of the chalk-banks in which they ſeem to grow.

Speedy refrigeration confuſes the phenomenon.

Theſe are the ſeries of phenomenon attending the devitrification of greeniſh glaſs when it happens ſlowly; but if the effect is too rapidly produced, the appearances reſemble thoſe obſerved in the devitrification of bottle glaſs. The window glaſs here ſpoken of, is of that kind which contains very little of earthy matter, except wood aſhes. It muſt admit of many variations according to the differences in its compoſition.

Clear and ſimple glaſſes are not eaſily devitrified.

White or colourleſs glaſſes are very difficultly cryſtallized or devitrified. When they are well made, we may affirm that a long continued heat does not change them; but for this property it is neceſſary that they ſhould be compoſed merely of ſilex, with the preciſe quantity of flux for its ſaturation. In this caſe, a very long continued fire will no otherwiſe change them than by rendering them more yellow and hard.

(The Concluſion in our next.)

ADVERTISEMENT.

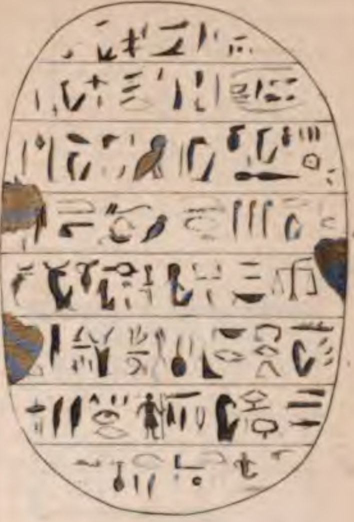
Various articles of Philoſophical News, Accounts of Books, and other ſubjects, have neceſſarily been poſtponed this month, on account of illneſs with which the Editor has been afflicted.

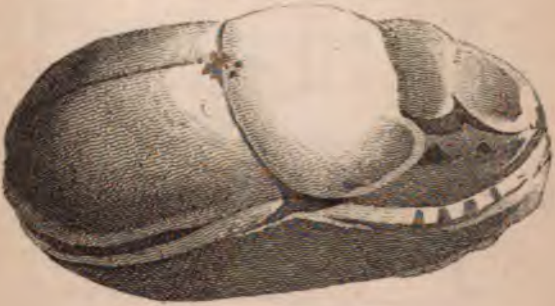
The ſame cauſe has alſo unfortunately prevented his Draughtſman from completing ſome Deſigns, which would elſe have appeared.

Mr. Ezekiel Walker has been prevented, by ſevere indiſpoſition, from anſwering the paper of C. L. in our laſt, upon the Horizontal Moon.

A note has alſo been received from C. L. in which he begs leave to notice, that the large lens mentioned in his experiment I. has a clear aperture of 6,1 inches in its frame, and was ſo uſed in that experiment.

Egyptian Engraving on Porphyry probably astronomical.





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Galvanic Illustrations.

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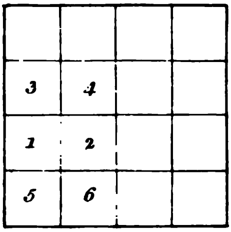

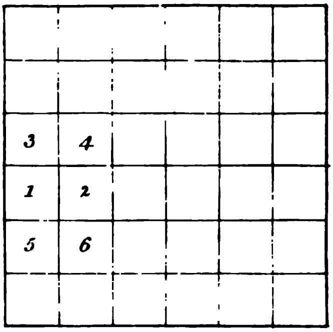

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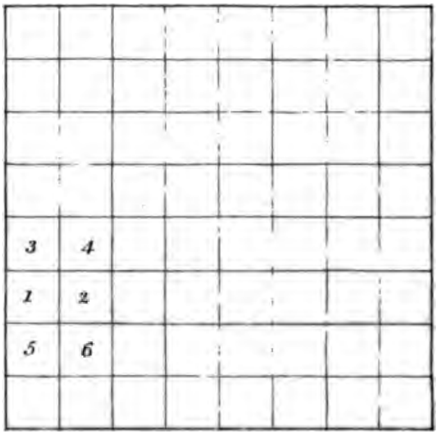

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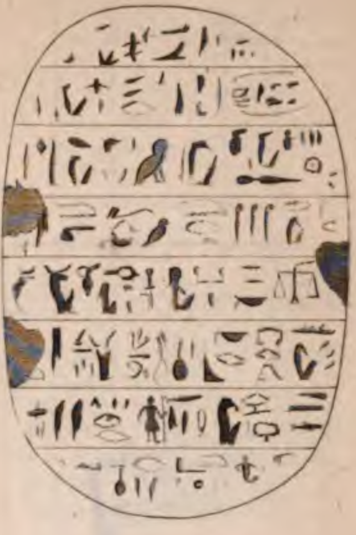

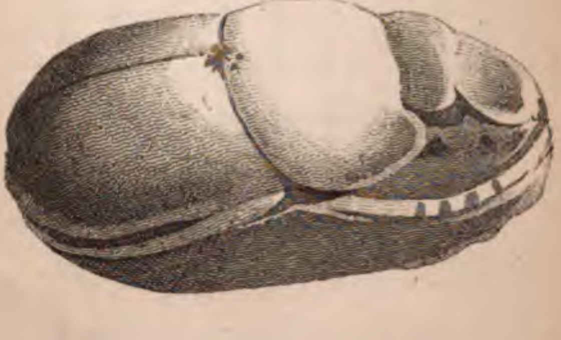

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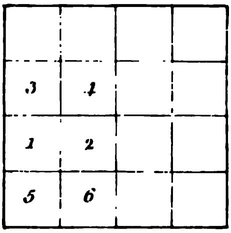
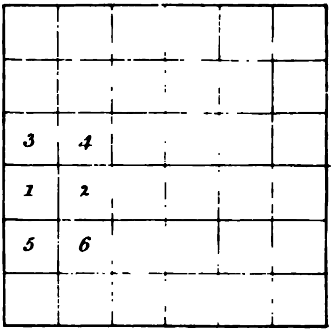
Galvanic Illustrations.

Fig. 1.





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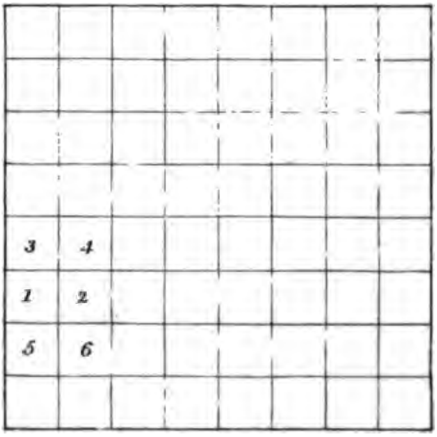

Fig. 3.

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THE ARTS.

FEBRUARY, 1805.

ARTICLE I.

Experiments and Remarks on the Augmentation of Sounds. In a Letter from Mr. JOHN GOUGH.

To Mr. NICHOLSON.

SIR,

Sounds heard farther from an extended ſurface.

A FACT is mentioned in a former communication of mine to your Journal (octavo, vol. III. p. 41) which proves, that a ſtroke given to an extenſive vibrating ſurface by a ſlender rod, produces a ſound poſſeſſing a high degree of force: from this it follows, that the range of a ſound may be extended by enlarging the vibrating ſurface, while the magnitude of the impulſe remains the ſame. The preceding maxim is of great uſe in acouſtics; but as the truth of this propoſition reſts at preſent on the authority of a local obſervation, it may not appear ſuperfluous to give a few eaſy experiments in confirmation of the fact.

Exp. 1. The beating of a watch audible at a greater diſtance when in contact with a metallic plate.

Experiment I.—My firſt attempt endeavoured to prove, that when a ſound ceaſes to be audible from remoteneſs, the enlargement of the vibrating ſurface will again render it diſtinct at the ſame diſtance. For this purpoſe, a watch was ſuſpended, on a calm day, from the branch of a tree, about 5½ feet from the ground. The clicking of it barely reached my ear, in thoſe intervals of ſilence which were not diſturbed by foreign ſounds, at the diſtance of 5½ yards; but when I removed half a yard farther, the ſtrokes of the balance were no longer heard. A circular plate of rolled iron, one foot in diameter, was then brought into contact with the part of the watch fartheſt from me, the poſition of it being ſuch as to preſent the plane of the circle to my ear. This circumſtance increaſed the range of the ſound; for every ſtroke of the inſtrument was diſtinctly perceived at the diſtance of four yards, when the ſilence of the place remained undiſturbed by other cauſes. The reſult of this experiment is too obvious to need a comment: and I have only to add, that when the trial is made within doors, the range of the clicking is greatly increaſed; becauſe the primary ſound receives an acceſſion of force from a number of its own pulſes, which are reflected to the ear by the furniture and walls.

Exp. 2. The plate gives a conſiderable ſound itſelf.

Experiment II.—I proceeded, in the next place, to examine the effects produced on the auditory organs by the vibrations communicated to an elaſtic ſurface by a watch; the immediate ſound of which was ſmothered by art. For this purpoſe, a watch was placed upon a cuſhion, under an inverted porcelain cup, which was wrapped externally in ſeveral folds of flannel. The inſtrument was heard with difficulty at the diſtance of one foot in this ſort of confinement; but when I placed the muffler with the watch under it, upon a ſquare mahogany table four feet broad, the clicking noiſe imparted by it to the top of the table, reached the ear very diſtinctly at the diſtance of four yards. The ſame watch, covered in like manner upon the iron plate mentioned above, reached my ear at the diſtance of 22 feet. The apparatus ſtood in this trial upon a round oak table, 24 inches in diameter.

The enlargement of the ſurface augments the ſound.

The preceding fact proves indiſputably, that the ſecondary vibrations of an elaſtic ſurface actually affect the ear, in thoſe caſes wherein the pulſes never reach the perſon of the hearers, which proceed from the part that is excluſively conſidered as the ſeat of ſound; in other words it proves, that the cauſe of a ſound is not confined to the obſtacle receiving the primary impulſe, but that it is propagated from the point of impact through the contiguous bodies. The firſt experiment alſo ſhews, that the enlargement of the vibrating ſurface encreaſes the force, which a given ſtroke exerts upon an ear placed at a given diſtance; conſequently the increaſed force thus imparted to a ſound, muſt be aſcribed to the co-operation of the vibrations communicated in the firſt experiment to the plate of iron; for by uniting with thoſe of the watch, they evidently augmented the power of the latter inſtrument.

Theory of the force of ſound augmented by the vibrations of a ſurface touching the ſonorous body.

A rational theory of the forces of ſounds may be formed, by help of the preceding obſervations, in the following manner: *Firſt*, Suppoſe the plane ABC *(Plate IV.)* to be one of the ſurfaces of an elaſtic body, and let it receive an impulſe at the point O; then O will recede from the ſtroke in a right line perpendicular to A B C, drawing after it all the contiguous particles, which, in their turn, will change the place of the next circle of particles. In this manner a circular dent O M E Q T F will be formed; the center of which O will be below the plane A B C; M, the higheſt part of the margin, will be above it; and the extreme circle E Q T F, bounding the whole, will lie in the plane. *Second*, Thus will a circular ſwell be formed on the plane, reſembling the circular wave produced by a ſtone dropped into water. A ſeries of theſe ſwells will follow the firſt, each of which, in ſucceſſion, will lie more remote from the center O than its predeceſſor. *Third*, The collective force of each ſwell is equal to that of the firſt impulſe; and it is diſtributed over the ſurface of a ring, having O for its center and O E for its breadth; *i. e.* all the rings are of equal breadths, but unequal diameter. *Fourth*, The effect of the ſtroke is thus progreſſively propagated from O to the more diſtant parts of the plane A B C, with an uniform velocity. *Fifth*, Let P be the place of the ear in the right line O P, perpendicular to A B C; alſo let G H I J K be any ſwell in the ſame plane; then will the effect of that ſwell be carried to the ear, in the conical ſhell O G I J K P; and the impulſe imparted to the auditory organs, through the medium of this ſhell, will be greateſt when the diameter of the ring O I is leaſt, and the contrary. *Sixth*, If A B C be a concave ſpherical ſurface, having P for its center, all the ſwells will ſucceſſively exert equal forces on the ear at P. *Seventh*, If O P be put equal to a, and f meaſure the force of a ſtroke at the diſtance 1; then let v equal the velocity with which this force is propagated from O over the ſpherical ſurface, and l the breadth of the rings covered by the ſwells, the force impreſſed on the ear at P in a given interval of time, is equal to $\frac{fv}{la^2}$. *Eighth*, Though time is infinitely diviſible in a mathematical ſenſe, M. Euler has ſhewn *, that the ear conceives it to conſiſt of indiviſible or elementary particles; conſequently all the forces which arrive at P during one of theſe elementary intervals, make a ſingle indiviſible impreſſion on an ear placed at that point; becauſe this organ cannot take cognizance of a ſmaller particle of time; therefore the force of a ſound, produced at the center of a hollow ſphere by the undulations of its ſurface, is truly denoted by the expreſſion $\frac{fv}{la^2}$. *Ninth*, In general, the force of a ſound, at any inſtant of its duration, is equal to the ſum of the forces exerted by thoſe pulſes, which ſtrike the ear in the elementary interval: Conſequently, if the diſtance between the hearer and the ſounding body be ſuch, that lines drawn from his ear to every point of the vibrating ſurface, may be conſidered as equal, the force of the ſound will be expreſſed by $\frac{fv}{la^2}$. *Tenth*, To ſhew the nature of this theorem by an example; ſuppoſe a bell A, the diameter of whoſe mouth $= 1\frac{1}{3}$, to be an octave below a bell B, the diameter of which $= 1$: alſo let A be heard twice as far as B, when each receives an equal ſtroke; the ratio of the breadths of the ſwells in A and B is required. Let V, v, be the velocities of the ſwells in A and B, L, l, the breadths of theſe ſwells; alſo let 2 and 1 be their reſpective ranges: then ſince f is given, $\frac{V}{4L} = \frac{v}{l}$, by the theorem; hence, as $V : 4v :: L : l$. Now the ſemi-circumferences of A and B are in the ratio of $1\frac{1}{3}$ to 1; but while the vibrations paſs over half the circumference of A, they paſs and repaſs over the ſame part of B: therefore, as $V : v :: 1\frac{1}{3} : 2$: hence alſo, as $L : l :: 1 : 6$. Q. E. I.

The principle extended to voices and other ſounds.

Should the principle explained above be admitted, the following concluſion muſt alſo be received as a neceſſary conſequence of it: the voices of animals, as well as the notes of muſical inſtruments, and the reports produced by blows given to leſs elaſtic ſubſtances, derive no inconſiderable portion of their reſpective forces from the vibrations of parts which are not directly concerned in the production of the primary ſound. This opinion appears to be countenanced by an experiment, which I have repeated at different times under various forms, and of which the following is the ſubſtance: If a wire be ſtretched by two pins fixed into a bad conductor, ſuch as a block of ſtone, the ſound produced by it is much weaker than that of an equal wire ſimilarly ſtretched upon a board, which is a better conductor. In like manner, if a circular piece of wood be ſtruck by a leaden ball conſtantly falling from the ſame height, the report will be heard at a greater diſtance, when the wood is placed upon a good conductor of ſound, than when it reſts upon a bad one. Theſe facts create a high degree of probability, that the leading maxim of this eſſay is applicable to ſounds of every deſcription, embracing ſuch as are continuous, as well as thoſe of a momentary duration.

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* Tentamen de Mus. Cap. I. Sec. 13.

Similar theory by Dr. Matthew Young.

Dr. Matthew Young's Enquiry into the Phenomena of Sounds has fallen into my hands ſince the foregoing remarks were committed to paper, and a partial peruſal of the work has convinced me, that the preſent theory has not all the claim to originality which I once ſuppoſed to be due to it. The juſtice and neceſſity of the preceding obſervation will appear from the Doctor's two theories of the ſpeaking-trumpet and echoes; in both of which he has made uſe of my leading principle with ſucceſs. The maxim, however, has been extended to a greater number of caſes by theſe obſervations; beſides which, an attempt has been made to demonſtrate the truth of it by experiment, and it is on the two circumſtances here ſtated that the merits of the preſent letter muſt reſt; for I am far from deſiring to rob the celebrated author of the enquiry of the honour due to his ſagacity.

JOHN GOUGH.

Middleſhaw, Jan. 4, 1805.

Obſervations

II.

Obſervations on the different Degrees of Facility with which Maſſes of the ſame Material admit of Changes in their Temperature; with Applications of the Facts to the Conſtruction of Pendulums, and Speculations upon various new Forms of pendulous Regulators of Time. In a Letter from J. WHITLEY BOSWELL, *Eſq.*

To Mr. NICHOLSON.

SIR,

Compound pendulums.

THE ſatisfactory communication on an ingenious improvement of the compound pendulum, in your Journal for December, has reminded me of ſome ideas which occurred to me on ſimilar ſubjects. In hopes that they may be of ſome uſe in a matter ſo intereſting in itſelf, and ſo important in its application, I ſend them for your opinion.

A very material point ſeems to have been omitted in all the conſiderations I have met with on the expanſion of bodies by heat; pyrometrical experiments being directed to that merely of different ſubſtances of the ſame ſize, but none being made on bodies differing in bulk.

Bodies are more or leſs ſpeedily expanded by heat according to their dimenſions and figure.

Though various bodies differ in their degree of expanſion by heat according to their materials, yet all are ſubject to certain laws on this point, depending on their dimenſions; for as bodies receive or communicate heat by their ſurfaces, and retain it in proportion to their bulk, it follows, that their mutability of temperature muſt depend on the ratio of their ſolid contents to their ſurfaces; and that the greater the ſurface in proportion to the bulk, the more readily will a body change its temperature; and on the contrary, the greater the bulk in proportion to the ſurface, the leſs will it be affected by the fluctuating heat of the ſurrounding medium.

Deduction of the effects.

The proportion which the ſurfaces of bodies bear to their bulk may be varied, either by altering their ſhape or changing their ſize. In the firſt reſpect it is ſufficient to note, that the flatter and longer any body is, the greater will be its ſurface in proportion to its maſs of matter, and *vice verſa*: the difference cauſed by the variation of bulk can be more eaſily calculated, as in bodies of ſimilar figures their ſurfaces are exactly as the ſquares, and their ſolid contents as the cubes of their diameters. From this, and what has been already premiſed, it will follow, that in bodies of different ſizes, ſimilarly ſhaped, and of the ſame ſubſtance, the capacity for heat will be as the cubes, and the mutability of temperature as the ſquares of their diameters or ſides; and that, therefore, the degree of the tendency of thoſe bodies to maintain an equality of temperature, may be eſtimated as their ſolid contents *minus* the relative value of their ſurfaces: Bodies whoſe ſhapes are diſſimilar, will in ſome degree be ſubject to the ſame rule; but it is not material to the preſent ſubject to take them farther into conſideration.

Table of the rapidity of heating and cooling.

The following table of the proportion of four cubes in the above reſpects, whoſe ſides are reciprocally as 1, 2, 3, and 4, will exemplify what has been above aſſerted, and ſhew how greatly the retention of temperature of bodies is increaſed by adding to their bulk.

	Side.	Surface.	Change of Heat as	Bulk.	Capacity for Heat.	Retention of Heat eſtimated.
	Feet.	Square Feet.		Cubic Ft.		
A	1	6 : 1	1	1	1	1
B	2	24 : 4	4	8	8	4
C	3	54 : 9	9	27	27	18
D	4	96 : 16	16	64	64	48

Inferences.

From theſe conſiderations the following inferences may be drawn: 1*ſt*, That the greater the bulk of any body, the leſs will be its mutability of temperature in proportion, and of courſe the leſs will it alter its degree of expanſion: 2*d*, That a large globe in the firſt place, or a cylinder, whoſe height was equal to its diameter, in the next place, or in the third place a large cube, would have its dimenſions very little changed by the fluctuations of atmoſpherical temperature.

Application of theſe facts to clocks.

To apply theſe principles to the regulation of horological movements may appear difficult, as the firſt idea that would occur is, that it would be neceſſary to put thoſe large bodies in motion for this purpoſe; but this is by no means needful, and it may be effectually performed by conſtructing and fixing up a pendulum in ſuch a manner, that its variations in length ſhall be corrected by, and depend on, thoſe of the large body.

There

Pendulums of Croſthwaite and Pine.

There is no need of much inveſtigation to diſcover the mode of doing this; as either the pendulum invented by Mr. Croſthwaite, of Dublin, or that contrived by Mr. Pine, will fully anſwer this purpoſe, though they will by no means perform that for which they were intended,---of effecting a compenſation of themſelves, in the manner of the gridiron pendulum.

Their miſtake.

The error of theſe gentlemen, in this reſpect, is very fully pointed out in a paper ſigned A. B. publiſhed in the ſeventh Volume of the Repertory of Arts, which would have been much more creditable to its author, had he not triumphed ſo much in his ſuperior penetration on the occaſion, which has led him to forget himſelf ſo far as to deſcend to the illiberality of national reflection on the part of Mr. Croſthwaite: for which reaſon I own I am happy to have it in my power to give theſe gentlemen the ſatisfaction of ſeeing an error pointed out in his paper in return; which is, where he aſſerts that a ſmall rod of mahogany muſt have the ſame contraction and dilation, from change of heat, as a *ſolid plank* of the ſame wood, or words to that effect, the fallacy of which opinion appears from what has been here laid down: It would be eaſy to diſtinguiſh this philoſophy by an appellation of the ſame ſtamp as that which A. B. has beſtowed on Mr. Croſthwaite's invention, and the more unfortunately for his inſinuation on this occaſion, as the Iriſh method happens to be the beſt of the two; for Mr. Croſthwaite ſupported his pendulum by a ſolid wall, which would be much leſs affected by change of heat in the air than the back of the clock-caſe to which Mr. Pine attached his.

Uſe of a large block of ſtone for fixing a pendulum and clock.

The beſt method of applying the foregoing facts to the regulation of clocks appears to me to be, to procure a large cylinder, or octagonal priſm, of ſtone, the diameter of whoſe baſe is equal to its height, and exceeds the length of the pendulum by ſome inches at leaſt, and as much larger as it could be got the better: one of a cubic ſhape would alſo do, if its maſs of matter exceeded that which could be got of the other forms; for this purpoſe granite ſtone ſeems preferable, and in the next place Portland ſtone, as they can be eaſily had in large blocks; but it is probable that ſtones which grow damp in moiſt weather would not be ſo proper for this purpoſe. To this block of ſtone a pendulum ſhould be attached with a ſingle compenſating rod added to it, in the ſame manner as that conſtructed by Mr. Croſthwaite or by Mr. Pine: the clock itſelf ſhould alſo be faſtened to the ſtone, particularly if Mr. Croſthwaite's pendulum is uſed.

Defence of the block from change of temperature, &c.

The block of ſtone may be farther ſecured from change of temperature, by being ſurrounded on every ſide by brick-work, except where the pendulum and clock are fixed, and having dry ſaw-duſt rammed in between it and the brick-work. It is probable that a block thus fitted up, would vary little from changes in the heat of the air; and in thoſe ſituations where large blocks of ſtone are found naturally, this might be done at a ſmall expence: where a large block of ſtone could not be procured, a piece of dry ſtraight-grained red deal balk, or ſolid mahogany, might be uſed to good effect, if well painted, and incloſed as before directed: and for common uſe, it is probable that a large glaſs tube, filled with ſemen lycopodii, if to be had, or fine dry ſaw-duſt, well cloſed at either end, and covered with oiled ſilk, would form a pendulum-rod little affected by heat or cold; but if metal is preferred, then a tube of metal, fitted up as laſt directed, would be preferable to the ſmall wires now uſed for this purpoſe.

Advantages.

The advantages to be expected from the principal method above-mentioned, of fitting up pendulums, is, that it affords an unlimited mode of approximation to perfect compenſation; and that, as it requires no great refinement of workmanſhip in its conſtruction, it can be uſed in many ſituations where it would be impoſſible to have a tubular compenſating, or common gridiron pendulum made; which, notwithſtanding the late improvements, are extremely difficult to conſtruct with accuracy, as may be ſeen in the paper publiſhed in your Journal for December laſt, where many of the impediments to their perfection are pointed out, and more ſtill remain to be noticed, of which the following deſerve ſome attention.

Imperfections of compound pendulums ſtated. The metals are mixed, and not conſtant in their compoſition.

The metals of which gridiron pendulums are compoſed, are both of them mixed metals, and of courſe every different parcel of them made, muſt vary in ſome degree in the relative quantities of their component ingredients, and from thence in their degree of expanſion by heat. It may appear ſtrange to call ſteel a mixed metal; but when it is conſidered that charcoal forms a conſiderable part of it, the juſtice of eſtimating it as ſuch will be evident; and in this metal the proportion

portion of charcoal in different parcels of it, is much more various than that of zinc is in braſs: perhaps, for this reaſon, it might be better to uſe ſome of the ſimple metals in the place of theſe, in their conſtruction.

Difficulty of adjuſtment.

Another difficulty in forming theſe pendulums with accuracy, ariſes from the want of a good method of proving them. The adjuſtment by the going of a clock is ſhewn to be imperfect in the paper on the tubular pendulum, and that propoſed in its place, the uſe of the pyrometer, is equally defective, for the following reaſons:

Defects alledged in pyrometers.

Pyrometers are unfit for meaſuring the effects of the heat and cold of the atmoſphere on any ſubſtance, becauſe their own machinery, and particularly the part ſupporting the ſubſtance under trial, is liable to be affected by the ſame cauſes: and in this caſe the index will ſhew the ſum of the alteration of the ſubſtance, the part of the apparatus between its two extremities, and the machinery of the pyrometer, inſtead of that of the ſubſtance alone: and in applying artificial heat to the matter under trial, it is extremely difficult to communicate that heat equally to all its parts at the ſame time, and ſo as not to operate on the pyrometer itſelf, without which the exactneſs of a compound pendulum could not juſtly be tried by it.

Laſtly, another error is produced in the computation of the aberration of all pendulums, from not taking into account the dilation and contraction of the ſmall ſteel ſpring by which they are uſually ſuſpended.

Other pendulums.

As all the oſcillatory pendulums yet made public are influenced in their notation of time by the expanſion of their ſubſtance, and as the beſt contrivances to compenſate this are only an approach to perfection, an increaſe of probability but no certainty, it is therefore an object worthy attention to inveſtigate other methods for effecting the ſame purpoſe: for which reaſon I ſhall here beg leave to ſuggeſt the reconſideration of a ſpecies of pendulum of a different nature, which has never been condemned on any juſt ground that I could hear, and which in fact is ſo little known, that the application of a ſimilar movement to other purpoſes, is by many ſuppoſed to be a late invention. The pendulum which I allude to, is that treated of in the fifth Part of the *Horologium Oſcillatorium* of the well known Huygens, publiſhed in 1673, which he calls

Circular pendulum of Huygens.

calls the circular pendulum, and which much reſembles the centrifugal apparatus, uſed frequently about ſteam-engines to regulate the aperture of the ſteam-damper, of which it probably ſuggeſted the firſt idea. To form the circular pendulum, a ſpindle (H D, *Fig.* 1, *Plate III.*) proceeds perpendicularly from the clock-work, from whence it receives a circular motion round its own axis; to this is affixed a lamina (B G A) of ſome breadth, bent according to a paraboloidal line, by the evolution of which (after it is joined to a certain right line) a parabola is formed, the conſtruction of which is ſhewn in the eighth propoſition of the third part of the above work: This lamina cauſes the ball of the pendulum (F), attached by two threads to its upper extremity (as it circulates), to perform all its revolutions (which will be of greater or leſs extent as the axis moves with greater or leſs force) in the ſurface of a conoidal parabola (F E). Huygens declares, that all the circles performed by the pendulum thus conſtructed, will be iſynchronous, and then ſhews how to proportion the parts of the apparatus, ſo as that each of its revolutions ſhall be performed in a ſecond, or in half a ſecond: He ſays, the only reaſon which cauſed the oſcillatory pendulum to be preferred to this was, that this laſt was more difficult to conſtruct. As this circular pendulum ſeems to poſſeſs the valuable property of correcting the alterations cauſed in it by expanſion, or of rendering them of no conſequence, the difficulty of conſtruction is a matter of no conſequence, eſpecially as there are now to be found artiſts ſo much more excellent in works of this kind than formerly.

Huygens gives the following character of this pendulum, which I tranſcribe in his own words: "*Plura tamen, hujus quoque generis de quo nunc loquimur, nec ſine ſucceſſu, conſtructa fuere: eſtque in his ſingulare illud, quod continuo atque æquabili motu circumferri cernitur index poſtremus, qui ſecunda ſcrupula deſignat, cum in omnibus aliis horologiis ſubſultim quaſi feratur: Item hoc quoque, quod abſque ſtrepitu ſonoque omni moveantur hoc ratione conſtructa automata.*" He concludes the book with thirteen theorems, *De vi centrifuga ex motu circulari*, ſeveral of which prove this kind of pendulum to have moſt valuable qualities: the ſixth of theſe, being very remarkable, I copy for your readers, who may not have an opportunity of ſeeing the work which contains it, as follows:

"*THEOREMA.*

" THEOREMA.

" *In cava ſuperficie conoidis parabolici, quod axem ad perpendiculum erectum habeat, circuitus omnes mobilis, circumferentias horizonti parallelas percurrentis, ſive parvæ ſive magnæ fuerint, æqualibus temporibus peraguntur: quæ tempora ſingula æquantur binis oſcillationibus penduli, cujus longitudo ſit dimidium lateris recti parabolæ genitricis.*"

From the above theorem, and what has been already laid down on the ſubject, it is evident that the circular pendulum is well worthy of a fair trial, and ſhould be recommended to the attention of all ſcientific conſtructors of horological movements.

As this circular pendulum will take up more room than a common one, when this is any conſiderable inconvenience, one of the following conſtructions on the ſame principles (which have occurred to me while writing this communication), may be uſed in its place.

Various conſtructions of the circular pendulum.

The firſt is as follows: To a ſhort ſpindle aſcending from the clock, as before deſcribed, let there be attached a glaſs or iron tube, bent in the form of the conoidal parabola beforementioned, and placed as E F in *Fig.* 1: into this tube let there be poured a ſufficient quantity of mercury to ſerve as a centrifugal weight.

It appears to me that the mercury riſing in the tube, as it circulates, by the centrifugal force, along its parabolical curve, will have the ſame effect as the weight in Huygens's conſtruction, cauſed by other means to move in a ſimilar line. The tube ſhould be ſufficiently large to admit the air to paſs freely above the mercury as it moves along its cavity, or elſe a ſmall aperture ſhould be made in the upper ſurface of the tube near the ſpindle, for the ſame purpoſe: to prevent alſo the error which might otherwiſe be cauſed by a varied reſiſtance to the motion of the circulating tube from the mutability of atmoſpherical preſſure, the tube may be incloſed in a circular veſſel covered at top, made of as light materials as poſſible, and very ſmooth externally, through whoſe axis the ſpindle ſhould paſs, and to which it ſhould be united ſo as to revolve with the tube. A ſecond method of effecting the ſame purpoſe is, to have a ſemi-cylindrical trough ſhaped and placed in ſame manner as the above tube, in which ſhould be put a metallic ſphere of ſmaller diameter than the trough, that the ſphere might move in it without friction; for, being thus formed, it follows, from the 13th of the third book of Euclid, that the ſphere could never touch the trough but in one point alone at the ſame time. A third method of conſtruction is, to place a cylindrical metallic rod, or thick wire, bent into the parabolical curve before directed, in the ſame poſition as the above tube and trough; with a ſpherical weight put on it ſo as to move freely along it, which laſt might be effected by a friction-roller, either let into the upper part of the weight, or placed above it, whoſe ſurface ſhould be formed into a circular groove of larger diameter than the rod.

Thoſe laſt conſtructions ſeem to me to have beſides the farther advantage, that any dilation of their parts from change of temperature, could only lengthen the tubes or rods, but would not alter their ſhape, on which alone their properties depend; which circumſtance might perhaps make them even more exact than that propoſed by Huygens, the ſtrings, or appending part of which, would certainly vary in length; which, though, from the before-recited theorem, it appears to be a matter of little conſequence, yet Huygens ſeems to hint that it would be of ſome, by recommending the uſe of fine chains inſtead of ſtrings (that their length might vary the leſs), in theſe words: "*In locum fili catenulam tenuem exauro, aliove metallo, adhibere licebit, quo melius invariata ſervetur longitudo.*"

An oſcillatory or rolling pendulum.

There has alſo occurred to me a ſpecies of oſcillatory pendulum; which, as it appears to have the ſame property as thoſe laſt mentioned, (of not being affected by the expanſion of its materials,) I ſhall here deſcribe: It conſiſts of a cylinder of hard metal, A (*Fig.* 4) turned very true and ſmooth, placed ſo as to roll back and forwards alternately, in the cycloidal cavity B, cut in a block of hard metal alſo, and well poliſhed: the communication of motion between the rolling cylinder and the clock work to be effected by the rod C, ſuſpended by its upper extremity, and connected with each extremity of the axis of the cylinder by the joints D D: The diameter of the generating circle of the cycloid being half the length of a pendulum rod, which vibrates in the required time, and the cycloidal part well levelled and firmly ſuſtained. It ſeems to me that the cylinder A, when put in motion, will oſcillate equal times on its cycloidal ſupport; and, (as the expanſion of the latter will not alter its ſhape, on which alone its properties depend,)

pend,) that the reſult will be the ſame in every different temperature; B may alſo be formed of two cycloidal plates, having circular grooves hollowed on their edges, and A have two projecting circles turned on it to run in thoſe grooves; the method of connecting A with the clock-work may alſo be varied many other ways.

Concerning friction, &c.

Before I conclude, I beg leave to obſerve that the method, before mentioned, of preventing friction in the motion of the ſphere in the parabolical trough, might be applied very beneficially in the conſtruction of rail roads, by forming the upper part of the rail of a ſemi-cylindrical ſhape, and making in the edges of the wheels, (which were to run on them,) circular grooves, whoſe diameters ſhould each exceed that of the rail; in the uſual way of forming rail-roads, the lateral friction to the wheels is very conſiderable, whether they have flanches to them, and the rails are made plain, as in the Swanſea roads; or the rails are formed with riſing ledges, and the wheels plain, as in the Croydon road.

The circular pendulum a good regulator of firſt movers.

I ſhall alſo requeſt permiſſion to mention, in addition to your note on Prony's condenſer of forces, that a regulator might be formed on the principles of the circular pendulum, as *Fig.* 3, which would temper the motion of wind-mills, or other engines, and react on them in return, (as well as the fly wheel), in a much more ſimple and effectual manner than the very complicated apparatus deſcribed by Prony for the ſame purpoſe, and which ſeems to me moreover peculiarly liable to be broken by any ſudden increaſe of velocity in the mill, unleſs formed of moſt cumberous ſtrength.

I requeſt the favour of having the following typographical errors noted in the paper on my tallow lamp. November, 1804, page 147, line the eighth, for the word *freſh*, read *freely*; and line the 25th, ſame page, between the words *means* and *horizontal*, inſert the letter P. In the plate alſo, the pan ſhould be deeper and larger than there repreſented, and the tallow holder more central.

I am, Sir,

Your very humble ſervant,

J. WHITLEY BOSWELL.

Reference to the Figures.

Fig. 1. An exact copy of the figure of Huygens's circular pendulum, from the Horologium. B G A, the edge of the paraboloidal lamina: F E, the parabola produced by its means: F, the ball.

Fig. 2. The firſt conſtruction propoſed in place of *Fig.* 1. A C, the parabolical glaſs tube: A, the mercury: C, the air-vent: B B, the encloſing circular veſſel repreſented in ſection.

Fig. 3. The third conſtruction propoſed inſtead of *Fig.* 1. A C, the metallic rod bent into a parabolical figure: B, the ſpherical weight: V, the roller on which it moves along A C: the parts in this figure are repreſented double, to ſhew the beſt method of adopting this plan as a regulator for wind-mills and other engines.

Fig. 4. The propoſed conſtruction for an oſcillatory pendulum, which, it is imagined, will not be affected by change of temperature. A, the oſcillating cylinder: B, the cycloidal ſupport.

Fig. 5. A ſection of the trough mentioned in the ſecond propoſed conſtruction, with the ſphere in it.

Annotations. W. N.

The beſt pyrometers do not give fallacious reſults.

Deluc's method.

Pyrometers, p. 74.] The common pyrometers of the ſhops are indeed liable to the objections of our author; but ingenious men have long ago obviated them in their experiments. Deluc, in the Philoſophical Tranſactions for 1777, gives a method of aſcertaining the relative expanſions of two different metals by heat. He ſuſpended one of the bars to an arm proceeding horizontally from an upright deal plank, and he ſupported the other bar by reſting its lower end upon a ſmall cock or ſtage proceeding from the lower end of the former. A microſcope was attached to the plank in ſuch a manner that, while it was conſtantly ſupported by an horizontal arm, it could be moved ſo as to keep the laſt mentioned or ſtanding bar in the focus of radiation for diſtinct viſion. Heat was applied to the bars, and the microſcope was directed to a point on the ſtanding bar. When by repeated ſhifting the microſcope a point was found which was neither raiſed nor depreſſed by the changes of temperature, the reſpective lengths of the bars were inverſely as their expanſive powers. There was no mechaniſm, and the plank would remain at the ſame temperature during the experiments,

riments, or might be kept ſo by means ſimilar to thoſe pointed out by our ingenious author, or his block of ſtone itſelf might have been uſed.

Ramſden's pyrometer.

That great artiſt Ramſden, whoſe mechanical ſkill and clearneſs of intellect were ſo variouſly diſplayed in his works, and ſtill more in his converſation to thoſe who remember and regret that he has recorded ſo little of the reſults of his labours: this artiſt firſt availed himſelf of microſcopes for aſcertaining the terminal points of the ſubject under examination by his pyrometer; and the interval between one microſcope and the other was rendered permanent by fixing them to arms proceeding at right angles from a bar of iron, kept at 32° by ſurrounding it with melting ice. Phil. Tranſ. Vol. LXXV.

Methods of examining gridiron pendulums.

Magellan's inſtrument.

Laſtly, another error, p. 74.] I believe the ſmall ſpring and ſimple bar part, if any beneath the gridiron, are always taken into the account. Theſe pendulums have not unfrequently been ſubjected to actual examination. An old friend of mine, Mr. J. H. de Magellan had an apparatus of tin, (Anno 1784) in which he incloſed the gridiron when to be examined. It was hung by its ſpring to an arm fixed in an upright plank of deal wood, againſt which plank were attached thermometers to ſhew its ſteadineſs of temperature; and from the center of oſcillation proceeded a (temporary) rod, the end of which preſented a dot as the object to be viewed through a microſcope duly attached to the plank. Without dwelling minutely on this apparatus, I need only obſerve that neither the arm of ſuſpenſion, nor the ſupport of the microſcope, nor the rod from the center of oſcillation had any parts but ſuch as were parallel to the horizon, while the pendulum itſelf was perpendicular to it, and conſequently that no error could ariſe but from expanſion in the plank, which remained at the common temperature; and that the tin apparatus which covered and encloſed the pendulum had only three openings, one at the bottom, one at top, and one through which the rod from the center of oſcillation paſſed, without touching the ſides. In the experiment, ſteam from boiling water was admitted below, and when the heat was ſo raiſed, and the ſupply kept up, that uncondenſed ſteam eſcaped above the pendulum was judged to be at 212°, and if the center of oſcillation continued before the microſcope without either riſing or falling, the compenſation was conſidered as complete.

Mr. Troughton's apparatus, alluded to in his paper is different from thoſe, and when he ſhall favour me with the account of it, and the experiments he is making, the world will ſee that it anſwers its purpoſe with ſuch facility and preciſion as do honour to his great ſkill in theſe ſubjects.

Troughton's pyrometer:

Circular Pendulum of Huygens.] The difficulties attending the conſtruction of this pendulum ſeem to be principally in the ſtring or chain. Perhaps fine metallic wire might deſerve the preference. It would not probably be difficult to bring the curve A B ſufficiently near the true figure to anſwer its general purpoſe, through the changes of force in the firſt mover, and of reſiſtance in the air. Practical men have not thought the cycloidal cheeks applied by Huygens, and ſince him by others, to the common pendulum, of any utility; but have rather directed their attention to ſmall vibrations, or an equalizing of the firſt mover by periodically detaching the train. Huygens himſelf alſo propoſed this expedient. It is generally admitted to be an advantage that the regulating inſtrument, whether pendulum or balance, ſhould perform the greateſt part of its motion unconnected with the train. The circular pendulum is conſtantly ſo connected.

Remarks on Huygens's circular pendulum.

As the ingenious author has not detailed his reaſons for thinking that pendulums of this kind will not be affected by change of temperature, I would ſimply remark that it does not appear to me why an expanſion of the ſtring ſhould not cauſe the pendulum of Huygens to revolve more ſlowly, or that an expanſion of the curves in the other figures would not diminiſh their curvature and produce the ſame retardation.

Whether it be leſs liable to change from temperature.

The tube may be encloſed in a circular veſſel.] As the reſiſtance of the air will vary no leſs than a tenth or a fifteenth part according to the ſtation of the barometer, we might expect time pieces to be conſiderably affected by its irregularity. In aſtronomical clocks, with heavy pendulums and ſhort vibrations, this quantity is extremely minute, as is proved by their very correct performance. But there are facts attending the performance of our beſt portable chronometers, which ſhew that it is not in them an inconſiderable object. Several years ago I entertained a notion that the reſiſtance of the air might be rendered equable, or in fact done away, by boxing up the balance, as is here propoſed by our author. But from his own excellent paper on the blaſt ventilator inſerted in the fourth

On the method of encloſing the pendulum, &c.

fourth volume of the quarto ſeries of this Journal, as well as from thoſe of Profeſſor Venturi, inſerted in the ſame work, I learned how greatly the lateral reaction of the air, which is put in motion by bodies paſſing through it, muſt affect their movements. From this cauſe the value of the contrivance will undoubtedly be diminiſhed, but I think not done away. I believe the famous pocket time-piece made about the year 1784, by Emery of Charing-Croſs, for the Preſident Saron at Paris, had its balance boxed up; but as I ſpeak only from recollection, it is quite as probable that the whole movement was encloſed in a cap or caſe ſhut up by a ſecret catch.

III.

Analytical Experiments and Obſervations on Benzoin. By Mr. William Brande. *Communicated by the Author.*[*]

Benjamin or benzoes little examined.

THE ſubſtance which forms the ſubject of the following experiments, and which is generally termed Gum-Benjamin or Benzoes, may be enumerated, among thoſe objects, which till lately, have but little engaged the attention of chemiſts.

Mr. Hatchett's experiments.

We are indebted to Mr. Hatchett for almoſt all that is as yet known, as to the chemical properties of the reſins, gum-reſins and balſams; and as the ſubſtance in queſtion, had not been examined by that gentleman, I was induced to make the following attempt.

Benzoin is obtained from the ſtyrax benzoe.

Benzoin is obtained by inciſions made in the tree called *Styrax Benzoe* [†], from whence the balſam flows, one tree ſcarcely ever yielding more than three, or at the moſt four pounds.

Two kinds: viz. benzoë amygdaloides, and

Benzoin is generally divided into two kinds; the one is formed of whitiſh yellow tears, reſembling almonds, united together by a brown ſubſtance of the ſame nature, and is diſtinguiſhed by the name of benzoë amygdaloides.

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[*] The following experiments were made merely with a view of aſcertaining the effects of different menſtrua on benzoin; but as the action of ſome of the acids, &c. would have been much leſs intereſting, without a knowledge of the component parts of the balſam, I was induced to make ſome analytical experiments, which, though I fear they are by no means ſo accurate as they ought to be, will, I hope, ſtill be found of ſome uſe.

[†] Dryander has given a deſcription and drawing of this tree, *vide* Phil. Trans. 1787. p. 307.

Common benzoin.

The other is a brown ſubſtance, ſomewhat reſembling common reſin, but poſſeſſing in other reſpects, the ſame qualities as the former; it is called common benzoin.

Cauſe of this difference.

The difference between theſe two ſpecies is ſaid to ariſe from the latter having been expoſed, for a length of time, to the ſun, which converts the white benzoin into the brown.

Benzoin of the ſhops.

The benzoin of the ſhops is uſually in very large brittle maſſes, and ſuch as is whiteſt, and free from extraneous ſubſtances is the moſt eſteemed. It grows in Sumatra, and is brought from the Eaſt Indies only.

Taſte. Smell.

When chewed, it impreſſes a ſlight ſweetneſs on the palate; it has however but little taſte. Its ſmell is fragrant and very agreeable, becoming much more perceptible when gently heated.

Effects of heat on benzoin. Sp. gr.

When thrown on hot coals it firſt fuſes, then takes fire, emitting at the ſame time a ſtrong penetrating odour. Its ſpecific gravity, according to Briſſon, is 1.092.

Compoſed of reſin and benzoic acid.

Gren enumerates it among the reſins *, as do moſt other authors of his time; it is however more properly denominated a balſam, ſince it is a combination of reſin and benzoic acid.

We ſhall firſt examine the effects of different menſtrua on benzoin, and then proceed to its analyſis, together with the methods of obtaining benzoic acid.

§ I.

Effects of different Menſtrua on Benzoin.

Effects of water on benzoin.

1. Cold water has but very little effect on benzoin; boiling water however extracts a part of its acid.

Of alcohol.

2. Alcohol diſſolves the whole of benzoin when digeſted with it in a very gentle heat, the impurities remaining behind. The ſolution is of a deep yellow colour, when perfectly ſaturated, inclining to reddiſh brown, and poſſeſſing in ſome degree the fragrant ſmell of the balſam, which may be obtained pure, either by gentle evaporation of the ſolution or by the addition of water, when a white powder precipitates, formerly known by the name of magiſtery of benzoin.

Action of the acids on this ſolution.

The actions of the acids on the ſolution of benzoin in alcohol, are very deſerving of attention.

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* Handbuch, § 1174. 2nd edition. 1794.

Of muriatic acid. A. Muriatic acid being added, a white curd is formed, and when three parts of acid and one of the ſolution are heated together, a very ſmall part of the balſam is rediſſolved, which ſeparates as the mixture cools, while the remainder is converted into a black brittle ſubſtance when cold, part of the benzoic acid having been diſſipated by the heat and part diſſolved, ſome of which ſeparates in the form of a white ſcum, as the mixture cools, together with the benzoin.

Of ſulphuric acid. B. Two or three drops of ſulphuric acid added to the alcoholic ſolution, occaſion a white precipitate, which diſappears on the addition of a ſmall quantity more of acid, the mixture aſſuming the appearance of port wine.

If however equal parts of the ſolution and of ſulphuric acid be mixed together, a dark pink precipitate is formed, the fluid becoming likewiſe of the ſame colour, but the addition of water changes the whole to a lilac colour.

On evaporating a mixture of equal parts of ſolution of benzoin and ſulphuric acid, the liquid retains its beautiful red colour till towards the end of the proceſs, when it becomes gradually darker, and if the evaporation be carried on to dryneſs, a black coaly ſubſtance remains, a decompoſition of the balſam having taken place.

Of nitric acid. C. Nitric acid added to the ſolution of benzoin in alcohol forms a dark red fluid, and when equal quantities are mixed, a violent efferveſcence takes place, attended with the emiſſion of much nitrous gas. It muſt be obſerved that in this inſtance, no precipitate is formed, which proves that the balſam is ſoluble in nitric acid.

Of acetous acid. D. The effects of acetous acid do not exceed thoſe of water.

Of acetic acid. E. Acetic acid forms a precipitate when added to this ſolution of benzoin. The effects produced by acetic acid on benzoin will be noticed hereafter.

Of the alcalies. F. The alcalies form no precipitate in this ſolution of benzoin, although the mixture in ſome caſes becomes ſomewhat turbid.

Action of ether on benzoin. 3. Ether diſſolves benzoin with great facility, the balſam being ſeparated, when this ſolution is agitated with water.

Effects of the acids on this ſolution. The effects of the acids on this ethereal ſolution, ſo nearly coincide with thoſe produced on the ſolution in alcohol, that there will be no need of a detailed account of them.

4. Nitric

Effects of nitric acid on benzoin.

4. Nitric acid produces a violent efferveſcence when poured upon powdered benzoin, the balſam being at the ſame time converted into an orange coloured maſs.

Solution.

Six ounces of nitrous acid, of the ſpecific gravity of 1.36 being poured on one hundred grains of very pure benzoin reduced into powder, the mixture being gradually heated in a ſand bath till it boils, a ſolution of a light yellow colour is formed. The ſolution thus obtained, depoſits a ſmall quantity of benzoic acid, as it cools: this quantity gradually increaſes, till after ſome days, the whole of the benzoic acid appears to have been ſeparated.

Separation of benzoic acid from,

firſt obſerved by Gottling.

This fact was firſt obſerved by Gottling; he does not however ſeem to have obſerved that the whole of the balſam is ſoluble in nitric acid.

Effect of water on the nitric ſolution,

of ſulphuric acid,

of muriatic acid.

A. When the above ſolution, recently made, is poured into water, the benzoin is precipitated, apparently unaltered.

B. Sulphuric acid cauſes no alteration in this ſolution.

C. Muriatic acid forms a white precipitate which is rediſſolved on the application of heat, forming a bright yellow liquid, which is a ſolution of benzoin in nitro-muriatic acid, and in which no precipitate is formed either by water or the alcalies. Its colour is converted into dark brown, when exceſs of potaſh is added.

Sol. of benzoin in nitro-muriatic acid.

Effects of water and the alcalies on the nitro-mur. ſolution.

Effects of ſulphuric acid on benzoin.

5. When ſulphuric acid is poured on pulveriſed benzoin, an efferveſcence takes place; part of the benzoin is diſſolved; forming a deep red liquid, and at the ſame time a coaly ſubſtance remains on the ſurface. Sulphureous acid gas is diſengaged during the ſolution.

Effects of the alcalies on this ſolution,

of water.

A. The alcalies form no precipitate in this ſolution, till after ſome hours ſtanding, when a dark coloured precipitate is formed.

B. Water produces a lilac precipitate in the recent ſolution.

Effects of muriatic and acetous acids on benzoin,

6. Neither muriatic nor acetous acids have any effect on benzoin.

of acetic acid.

7. Acetic acid diſſolves benzoin, even in the cold. The ſolution formed by the aſſiſtance of heat becomes very turbid on cooling, owing to the ſeparation of the benzoic acid.

Effects of the alcalies on this ſolution.

A. The alcalies form a white precipitate in this ſolution.—The precipitate formed in the alcoholic ſolution, (*page* 11) is re-diſſolved after ſome hours ſtanding.

Effects of acetic ether on benzoin.

8. Acetic ether diſſolves benzoin, without the aſſiſtance of heat.

9. When

Effects of ſoda and potaſh on benzoin.

9. When a boiling lixivium of pure potaſh is poured on benzoin, a ſolution is immediately formed; the ſame effect is produced when pure ſoda is made uſe of. Theſe ſolutions are of a dark brown colour, and become turbid after ſome days expoſure to the air.

Effects of the acids on theſe ſolutions.

Effects of ammonia on benzoin.

Precipitates are formed in them by the acids, which are rediſſolved on the application of heat, when nitric or acetic acids are added in exceſs. Ammonia likewiſe diſſolves a ſmall quantity of benzoin, from which ſolution it is precipitated by the acids.

Effects of water on the alcaline ſolutions.

No alteration is produced when the alcaline ſolutions are largely diluted with water.*

§ II.

ANALYTICAL EXPERIMENTS ON BENZOIN.

Diſtillation of Benzoin.

Diſtillation of benzoin.

One hundred grains of very pure benzoin were put into a glaſs retort, to which a tubulated receiver, and pneumatic apparatus were adapted; a very gentle heat was firſt applied, which was afterwards gradually increaſed, till the bottom of the retort became red-hot. The products thus obtained were as follows.

Component parts of benzoin.

	Grs.
Benzoic acid - - - - - - - -	9.
Acidulated water - - - - - -	5.5
Butyraceous and empyreumatic oil - -	60.
Brittle coal - - - - - - - -	22.
And a mixture of carbonated hydrogen and carbonic acid gas, which may be computed at -	3.5
	100.0

The quantity of benzoic acid above mentioned is that which may be ſeparated by ſublimation only, for on treating the oil, (which amounts to 60 grains) with water, five grains more of acid may be obtained, ſo that 100 grains of benzoin contain

Oil - - -	55
Acid - -	14

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* For an account of Mr. Hatchett's experiments on the reſins, gum reſins, and balſams, ſee Thompſon's Syſtem of Chemiſtry, 2d edition, Vol. IV. page 305. for the reſins and balſams to page 328. And from page 341 to 347 for the gum reſins.

Chaptal's method of obtaining benzoic acid.

The greateſt quantity of benzoic acid is obtained by Chaptals method, which conſiſts in diſtilling all the products over together, and ſeparating the acid by means of boiling water.

Scheele's proceſs.

The acid, however, thus obtained, is by no means ſo pure as that procured by Scheele's proceſs *, which is certainly far preferable in many reſpects.

Gottling's proceſs for obtaining benzoic acid.

Gren's proceſs.

Another way of obtaining the benzoic acid, is that recommended by Göttling. It conſiſts in digeſting the balſam, in a gentle heat with carbonate of potaſh and precipitation by ſulphuric acid. Gren made uſe of carbonate of ſoda: but it muſt be obſerved that in both theſe proceſſes, the acid is by no means ſo pure as that obtained by Scheele's method, becauſe a far larger proportion of the benzoin itſelf is ſoluble in the fixed alcalies, than in lime water, (the latter only diſſolving a very ſmall portion, to which its brown colour is owing) and which

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* Scheele's proceſs is as follows: "Upon four parts of unſlacked lime pour 12 parts of water, and after the ebullition is over add 96 parts more of water; then put 12 parts of finely powdered benzoin into a tin pan; pour upon it firſt about ſix parts of the above milk of lime, mix them well together, and thus ſucceſſively add the reſt of the mixture of lime and water. If it be poured in all at once, the benzoin inſtead of mixing with it, will coagulate and run together into a maſs. This mixture ought to be boiled over a gentle fire for half an hour, with conſtant agitation; then take it from the fire, let it ſtand quiet for an hour, in order that it may ſettle; pour off the ſupernatent limpid liquor into a glaſs veſſel. Upon the remainder in the pans pour 96 parts of pure water, boil them together for half an hour, then take it from the fire and let it ſettle; add the ſupernatent liquor to the former; pour upon the reſiduum ſome more water, boil it as aforeſaid, and repeat the ſame proceſs once more. At laſt put all the reſiduums upon a filter, and pour hot water ſeveral times upon it. During this proceſs, the calcareous earth combines with the acid of benzoin, and ſeparates it from the reſinous particles of this ſubſtance. A ſmall quantity of the reſin is diſſolved by the lime water, whence it acquires a yellow colour. All theſe clear leys and decoctions are to be mixed together and boiled down to 24 parts, which are then to be ſtrained into another glaſs veſſel. After they are grown cold, muriatic acid is to be added, with conſtant ſtirring, till there be no further precipitation, or till the maſs taſtes a little ſouriſh. The benzoic acid which was before held in ſolution by the lime, precipitates in the form of a fine powder." Vide Thomſon's Chemiſtry, Vol. II. page 123.

which is precipitated by the diluted ſulphuric acid. The following table ſhews the quantity of benzoic acid, obtained by the different proceſſes, from one pound of benzoin.

	Oz.	Dr.	Scr.	Grs.
Scheele's proceſs - - - - -	1	6	2	19
Chaptal's - - - - - -	2	0	0	0
By boiling benzoin with water - -	1	0	0	10
By Gren's and Gottling's proceſſes -	1	5	1	10*

The acid of benzoin, firſt deſcribed by Blaiſe de Vigenere.

Benzoic acid is deſcribed by Blaiſe de Vigenere, as long ago as the year 1608, in his treatiſe on fire and ſalt; he called it flowers of benzoin, becauſe it was obtained by ſublimation. We are indebted to Tromſdorf and Lichtenſtein for many facts relating to this acid.

Properties of the Acid of Benzoin.

Taſte of the acid. Smell.

This acid when obtained according to Scheele's proceſs, is a light yellowiſh powder. Its taſte is hot and rather bitter. It poſſeſſes a ſomewhat fragrant ſmell, ariſing from a ſmall portion of the aromatic oil, which ſtill adheres to it, it has however been obtained without any ſmell by Gieſe.

Properties in general.

It reddens tincture of turnſole. It is volatilized by a gentle heat, being converted into a white ſmoke, which excites a very diſagreeable ſenſation in the throat. When melted, it becomes as fluid as water, and aſſumes a radiated ſurface on cooling. Its ſpecific gravity is 0.667.

Decompoſition.

When diſtilled in cloſe veſſels, a part of it is decompoſed, which is converted into oil and carbureted hidrogen gas.—

Effects of oxigen gas, &c. on this acid.

Tromſdorf † found that it was not altered by oxigen gas, nor by the ſimple combuſtibles or incombuſtibles.

Of ſulphuric acid.

It is ſoluble in ſulphuric acid, which it converts into a brown liquid, and from which it is ſeparated by the addition of water.

Of nitric and ſulphurous and nitrous acids.
Effects of muriatic acid, &c.

The ſame effects are produced by nitric acid, and by ſulphureous and nitrous acids. It is not acted upon by muriatic, oxymuriatic, or phoſphoric acids. It is eaſily ſoluble in alcohol, from which it is precipitated on the addition of water. Its affinities appear to be as follows:*—White oxide of arſenic, potaſs, ſoda, ammonia, barytes, lime, magneſia, alumina.

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* Of pure acid, obtained by treating the precipitate with boiling water.

† See Tromſdorf's experiments on the benzoic acid, in his Journal der Pharmacie.

Its affinities.

Properties of the Oil of Benzoin.

The oil obtained by the diſtillation of benzoin poſſeſſes a ſtrong empyreumatic odour; but when rectified by a ſecond diſtillation, its ſmell is exceedingly fragrant and pleaſant. Its taſte is acrimonious and very diſagreeable.

Smell and taſte,

When diſtilled with water, it imparts ſome of its taſte to that fluid. It is perfectly ſoluble in alcohol, the ſolution becoming turbid when water is added.

Imparted to water.
Solution in alcohol.

Sulphuric acid decompoſes it in part, when the mixture is heated. Nitric acid acts on it as on the eſſential oils in general, converting it into a reſinous ſubſtance. It is not affected by muriatic acid, but is partially ſoluble in acetic acid. It ſinks in water; and forms ſaponaceous compounds with the alcalies.

Action of ſulphuric acid.
Of nitric acid.
Of muriatic acid, and the alcalies.

Such are the general properties of the acid, and oil of benzoin, much more might undoubtedly be ſaid concerning them; but as the foregoing experiments, were not made with a view of inveſtigating their properties, it would be unneceſſary to give a long account of them in this place. It muſt be obſerved that the pureſt benzoin has been made uſe of throughout; and laſtly, that no traces of alcali, were found in the coal which remains after diſtillation.

Arlington-Street.

IV.

On the Devitrification of Glaſs and the Phenomena which happen during its Cryſtallization. By DARTIGUES.

(Concluded from Page 64.)

WHEN clear glaſſes contain a certain quantity of a neutral ſalt which the fire has not had time or force to diſſipate, it often happens, during ſlow cooling, that the appearances called greaſe, threads, bubbles, and ſpecks, are ſpontaneouſly and ſuddenly formed.

Imperfect glaſſes.

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* This table of affinities was formed according to the experiments of Tromſdorff. Vide Thomſon's Syſtems of Chemiſtry, Vol. II. page 126. 2nd edition.

Theſe accidents, their different cauſes and remedies, are treated at length in the firſt part of my work, where I ſpeak of the fonding of glaſs; but though the explanation of theſe facts entirely belongs to the theory of devitrification, it will be ſufficient in this place to mention the phenomena which ariſe from the preſence of the different earths.

Cryſtallization of lime in glaſs.

Colourleſs glaſſes contain more or leſs of lime, for the reaſons which I have mentioned in ſpeaking of the different compoſitions of glaſs. This lime, when in exceſs, gives cryſtals very well, as Loyſel remarks: they are eaſily known, and ſo abundant, that they abſolutely impair the tranſparency: they are priſms, which ſeem to float in the midſt of the paſte of glaſs, and tend to unite in ſtars differently figured: theſe priſms are all nearly of the ſame thickneſs, and two or three millimetres in length (about one-tenth of an inch).

in large maſſes.

When this cryſtallization takes place ſpontaneouſly upon large maſſes at the bottom of the furnace, the colour of the glaſs becomes darker, and inclines to black, by the mixture of a certain quantity of the aſhes. The ſtriated ſtars here ſpoken of, become more numerous, the more remote they are from the ſide in contact with the fire. Cryſtallizations at firſt inſulated, are ſoon ſucceeded by a maſs entirely cryſtallized, in which the vitreous character is no longer diſtinguiſhable.

Modified cryſtallizations in glaſs.

Theſe are the moſt common characters of cryſtallization; but others are frequently ſeen, which are certainly owing to accident, and well deſerve notice on account of their varieties*. I have ſome pieces of glaſs in which cryſtals are diſcernable, ſo fine, that they can ſcarcely be ſeen with a magnifier. They are priſms divergent from the ſame center, and forming ſtars, which frequently do not exceed one millimetre (or half a tenth of an inch) in diameter: their union reſembles a ſlight miſt in the paſte of the glaſs.

Some pieces have the aſpect of a ſaline cruſt applied to a foreign body, againſt which the glaſs was in contact. In ſome of theſe this cruſt being compoſed of ſtriated protuberances, appears to advance more and more into the glaſs itſelf.

Laſtly, there is another variety perhaps more curious than all the reſt, in which centers, or cryſtallizations reſembling peas, and almoſt ſimilar to grain, are ſeen in the midſt of the paſte of glaſs. They are ſmall ſpheres flattened at each end with an indentation in the middle of each depreſſion. The ſides have facetts, like the grain of Indian naſturtium, and theſe faſſetts are always ſix in number.

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* Mr. Sage poſſeſſes a piece of glaſs cryſtallized in ſix-ſided baſaltic priſms, totally devitrified.

I intend, when I ſhall have procured a ſufficient number of this ſingular ſpecies of cryſtal, to analyze it, in order to determine which of the earths it is that effects ſo extraordinary a form.

I have thus given a ſhort account of the principal facts which characterize the precipitation and cryſtallization of glaſs. It is evident that they are of the ſame nature as thoſe produced by the cementation pointed out by Reaumur; and that this cementation is always a more or leſs abſolute devitrification of the glaſs.

Recapitulation.

When the glaſs is devitrified, it has no longer a vitreous but a granulated fracture; it has no tranſparency, and perfectly reſembles a ſtone; it becomes a leſs bad conductor of heat and of electricity; laſtly, it is no longer ſuſceptible of fuſion at the ſame degree of heat; and in order to reſtore it more eaſily to the vitreous ſtate, it muſt firſt be pulverized, to bring into contact thoſe ſubſtances which, during the cryſtallization, had become ſeparated from each other, and could no longer ſerve as fluxes to each other.

Revitrification.

General concluſion and application to geology.

I hope the novelty of many of the facts I have here indicated, and the conſequences I have drawn from them, may be ſufficiently intereſting to philoſophers to have ſome claim to their attention. I have no doubt but that other general inductions will preſent themſelves to them which may have eſcaped me, or which could not be introduced in a ſhort memoir. The reſemblance of my ſpecimens of devitrified glaſs with thoſe of certain lavas; the poſſibility that other lavas may have undergone a more complete devitrification by a much ſlower cooling beneath the volcanic currents which are known to have flowed, or to have remained fluid for whole years beneath congealed cruſts;—every fact leads me to think, that theſe phenomena may afford a ſolution of many geological events, concerning which no general opinion has yet been adopted, becauſe there was no reaſon to believe that ſtones may have formerly poſſeſſed the vitreous ſtate.

V.

Letter from a Correſpondent, relating to the apparent Reflection of Cold and the Invention of the Teleſcope, as noticed by Sir H. C. ENGLEFIELD *in laſt Month's Journal.*

To Mr. NICHOLSON.

SIR,

IN juſtice to Profeſſor Pictet, I truſt you will have the goodneſs to inſert in your next number the words of a note in his *Eſſai de Phyſique*, p. 81, where he deſcribes his experiment on the apparent reflection of cold:

Apparent reflection of cold.

" *Les Académiciens* del Cimento *avoient eſſayé de concentrer le froid au foyer d'un miroir concave; mais ils avouent eux-mêmes, que leur expérience avoit été faite d'un manière trop inexacte pour qu'on pût en rien conclure de certain.*"

Perhaps Profeſſor Pictet ſpeaks too lightly of the experiment of the Academicians; but ſo far was that experiment from being forgotten, that the account of it has been very lately reprinted in the firſt volume of the Journals of the Royal Inſtitution, p. 224; a work to which your correſpondent, who has revived the ſubject (page 1 of your laſt Number), has himſelf contributed ſome valuable papers.

Kepler did not invent or know of the teleſcope before Galileo.

With reſpect to the invention of the teleſcope, give me leave to obſerve, that *perſpicillum* means ſimply the ſight of an aſtronomical inſtrument, having a ſmall perforation. If the comet in queſtion was ſurrounded by a ſmall round and well-defined nebuloſity, ſuch a ſight might eaſily produce the effect attributed to it. A teleſcopic ſight is uſually called *perſpicillum vitreum*. Galileo ſays, in a work publiſhed in 1610, that he was informed of the invention of teleſcopes about nine months before. Kepler wrote, in 1611, a valuable work on the theory of dioptrics, in which the conſtruction of the aſtronomical teleſcope is deſcribed. Had he been the original inventor of the Galilean teleſcope, he would not omitted this opportunity of ſubſtantiating his claim to ſo important a diſcovery.

I am, Sir,
Your very obedient ſervant,
ALETES.

VI.

Facts tending to decide the Queſtion, at what Point of Temperature Water poſſeſſes the greateſt Denſity. In a Letter from Mr. John Dalton.

To Mr. NICHOLSON.

SIR,

ACCORDING to the doctrine in moſt of our books treating of the elements of chemical ſcience, it is conſidered as a decided fact, that water is of the greateſt denſity at or near 40° of temperature of Fahrenheit's ſcale; and that, above or below that temperature, it expands alike by heat or cold for a certain number of degrees. I have taught a different doctrine for ſome time paſt; which is, that water is denſeſt at 32°, or the freezing point, and that it expands alike above and below that point for at leaſt 25° (provided it does not freeze), and that the quantity of expanſion is as the ſquare of the temperature, reckoned from 32°, up or down; that is, if the expanſion from 32 to 42° be denoted by 1, that from 32 to 52° will be denoted by 4, and that from 32 to 62° by 9, *&c.* or nearly ſo; the deviation from ſtrict accuracy being, as I conceive, occaſioned by the mercurial thermometer not being an exact meaſure of temperature: the expanſion at 22° is likewiſe 1, at 12° it is 4, and at 2° it is 9, *&c.*; the ſame below as above the freezing point. In conſequence of this, a gentleman of profeſſional eminence has undertaken to examine the ſubject anew; and has been led, by an ingenious train of experiments totally different from the following, to decide in favour of the common notion, that water is denſeſt about 40°. Theſe experiments of his will ſoon be publiſhed. I am ſtill, however, decidedly convinced, that my opinion above ſtated is true, and that principally from the facts I am about to ſtate, which thoſe who adopt the common opinion muſt ſee the abſolute neceſſity of explaining upon their principle, or otherwiſe, of controverting the facts themſelves. As this ſeaſon of the year is moſt convenient for experiments of this nature, I am the more ſolicitous to have the facts announced, leaving all explanation for the preſent. They are of a very ſimple nature, and may be inveſtigated with little trouble and expence.

General notion that water is denſeſt at 40°, &c.

The author's doctrine that 32° is the point of greateſt denſity.

Experiments brought to ſupport the former concluſion.

The author retains his opinion, and invites other experiments.

A number

Inſtructions for experiments. Procure large bulbed thermometers, of earthen ware, glaſs, metal, &c.

A number of water thermometers are to be procured, the containing veſſels of which are of different materials, as earthen ware, glaſs, and various metals. Each of theſe ſhould contain one or two ounces (from 4 to 800 grains), more or leſs, of water. Common brown inkſtands, which go by the name of Nottingham ware, anſwer very well for one ſpecies, but they require to be well painted without, as they are not otherwiſe water-tight. I have a few of Queen's ware, made purpoſely in Staffordſhire, which conſtitute another ſpecies of earthen ware; ſome of them are glazed in and out; others unglazed, but theſe being painted without are made water-tight, and expand the ſame by heat as glazed ones: Of glaſs, common thermometer tubes, with larger bulbs than ordinary, are ſufficient. I have the metallic veſſels made in the ſhape of cylindrical tin canniſters, conical towards the top, and at the ſummit a ſmall cylindrical tube, ſuch as to take a thermometer tube. The glazed earthen ware and the metallic, require moſtly to be painted before they are quite tight.

Fill them with boiled water.

The veſſels being thus prepared, they are to be filled with water previouſly boiled to expel the air; a thermometer tube with cement is then ſuddenly plunged into the veſſel and cemented faſt; the water may then be driven out of the tube by heat, or more may be put into it by a ſmall wire; it is then fit for uſe, and a ſcale of equal parts may be applied to the tube; or it may be divided, and marks made with a file or paint.

Some of the reſults of my experiments with inſtruments of this kind, are as follow:

Experimental reſults noted with three earthen, one glaſs, two iron, one copper, one braſs, one pewter, and one leaden thermometer.

	Water loweſt.	Water the ſame height.
1. Brown earthen ware, No. 1,	at 36°	at 32° and 40°
2. Brown earthen ware, No. 2,	38	32 and 44
3. Queen's ware,	40	32 and 48
4. Flint-glaſs,	41½	32 and 51
5. Iron, thin plate,	42½	32 and 53
6. Tinned iron,	42½	32 and 53
7. Copper,	45½	32 and 59
8. Braſs,	46	32 and 60
9. Pewter,	46	32 and 60
10. Lead,	49½	32 and 67

Another

Phenomenon of the depreſſion of a thermometrical fluid when the inſtrument is ſuddenly heated.

Another phenomenon in theſe inſtruments is obſervable; it is not new, but it deſerves a marked attention in the preſent enquiry: If the apparent expanſion of water for the firſt 10° of temperature, reckoned from the loweſt point in any of the above inſtruments, be denoted by 1; then if the inſtrument, taken at any temperature, be ſuddenly plunged in water of 10° higher temperature, the water inſtantly *ſinks* a conſiderable way, occaſioned no doubt by the veſſel being extended by the heat before the water it contains has time to expand. The quantity of depreſſion in the different inſtruments was found as under:

Table of this depreſſion in the different thermometers.

Brown earthen ware ſinks by being dipped in water of 10° higher temperature, -	.2
Queen's ware, - - -	.3
Flint-glaſs, - - - -	.25
Iron, - - - - -	.66
Copper, - - - -	.9
Braſs, - - - -	1.1
Pewter, - - - -	1.0
Lead, - - - -	1.5

I ſubmit theſe facts to the conſideration of thoſe who feel intereſted in the enquiry; and deſire that they may be particularly inveſtigated by thoſe who maintain that water is denſeſt at 40°.

I remain your's,

J. DALTON.

Mancheſter, Jan. 10, 1804.

VII.

Analytical Experiments and Obſervations on Lac. By CHARLES HATCHETT, *Eſq. F. R. S. From the Philoſophical Tranſactions for* 1804.

(Concluded from p. 54.)

Properties of the colouring Extract of Lac.

Colouring extract of lac.

1. WHEN dry, it is of a deep red colour, approaching to purpliſh crimſon.

2. Being put on a red-hot iron, it emits much ſmoke, with a ſmell ſomewhat reſembling burned animal matter, and leaves a very bulky and porous coal.

3. Water,

Colouring extract of lac.

3. Water, when digeſted with it in a boiling heat, partially diſſolves it; but the reſiduum was found to be abſolutely inſoluble in water.

4. Alcohol acts but ſlowly on it; and, in a digeſting heat, diſſolves leſs than water. The colour of the ſolution is alſo not ſo beautiful; and a conſiderable part of the reſiduum left by alcohol was, when digeſted with water, found to be ſoluble, although this was not the caſe, when the reſiduum left by water was treated with alcohol.

5. It is inſoluble in ſulphuric ether, excepting a very ſmall portion of reſin, which appeared to be accidentally mixed with it.

6. Sulphuric acid readily diſſolves it, and forms a deep browniſh-red ſolution, which, being diluted with water, and ſaturated with potaſh, ſoda, or ammonia, becomes changed to a deep reddiſh-purple.

7. Muriatic acid diſſolves only a part: the ſolution is of the colour of port-wine, and, by the alkalis, is changed to a deep reddiſh-purple.

8. Nitric acid ſpeedily diſſolves it: the ſolution is yellow, and rather turbid; but the red colour is not reſtored by the alkalis, for theſe only deepen the yellow colour. This nitric ſolution did not afford any trace of oxalic acid.

9. Acetic acid diſſolves it with great eaſe, and forms a deep browniſh-red ſolution.

10. Acetous acid does not diſſolve it quite ſo readily, but the ſolution is of a brighter red. Both of the above, when ſaturated with alkalis, are changed to a deep reddiſh-purple.

11. The lixivia of potaſh, ſoda, and ammonia, act powerfully on this ſubſtance, and almoſt immediately form perfect ſolutions, of a beautiful deep purple colour.

12. Pure alumina, put into the aqueous ſolution, does not immediately produce any effect; but, upon the addition of a few drops of muriatic acid, the colouring matter ſpeedily combines with the alumina, and a beautiful lake is formed.

13. Muriate of tin produces a fine crimſon precipitate, when added to the aqueous ſolution.

14. A ſimilar coloured precipitate is alſo formed, by the addition of ſolution of iſinglaſs.

Theſe properties of the colouring ſubſtance of lac, eſpecially its partial ſolubility in water and in alcohol, and its inſolubility in ether, together with the precipitates formed by alumina and muriate of tin, indicate that this ſubſtance is vegetable extract, perhaps ſlightly animalized by the coccus.

The effects which it produced on gelatin, alſo demonſtrate the preſence of tannin; but this very probably was afforded by the ſmall portions of vegetable bodies, from which the ſtick lac can ſeldom be completely ſeparated.

Properties of the Reſin of Lac.

Reſin of lac.

This ſubſtance is of a browniſh-yellow colour; and, when put on a red-hot iron, it emits much ſmoke, with a peculiar ſweet odour, and leaves a ſpongy coal.

It is completely ſoluble in alcohol, ether, acetic acid, nitric acid, and the lixivia of potaſh and ſoda.

Water precipitates it from alcohol, ether, acetic acid, and partially from nitric acid; and it poſſeſſes the other general characters of a true reſin.

Properties of the Gluten of Lac.

Gluten of lac.

It has been already obſerved, that when ſmall pieces of ſhell lac have been repeatedly digeſted in cold alcohol, they become white, bulky, and elaſtic. By drying, theſe pieces become browniſh and brittle; the elaſticity is alſo deſtroyed by boiling water, exactly as when the gluten of wheat is thus treated.

If the pieces of ſhell lac, after the digeſtion in alcohol, be digeſted with diluted muriatic acid, or with acetic acid, the greater part of the gluten is diſſolved, and may be precipitated, in a white flaky ſtate, by alkalis; but, if theſe laſt be added to exceſs, and heat be applied, then the glutinous ſubſtance is rediſſolved, and may be precipitated by acids.

If the pieces of ſhell lac, after digeſtion in alcohol, be treated with alkaline lixivia, then the whole is diſſolved, and forms a turbid ſolution. But, when acids are employed, the chief part of the gluten is alone acted upon, and a conſiderable reſiduum is left, conſiſting of the wax, ſome of the reſin, and a portion of gluten, which has been protected from the action of the acid by the two former ſubſtances.

The above properties indicate a great reſemblance between this ſubſtance and the gluten of wheat; I therefore have called it gluten, but, at a future time, I intend to ſubject it to a more accurate examination.

Properties of the Wax of Lac.

Wax of lac.

If ſhell lac be long, and repeatedly digeſted in boiling nitric acid, the whole is diſſolved, excepting the wax, which floats on the ſurface of the liquor, like oil, and, when cold, may be collected; or it may be more eaſily obtained in a pure ſtate, by digeſting the reſiduum left by alcohol in boiling nitric acid.

The wax thus obtained, when pure, is pale yellowiſh white, and (unlike bees wax) is devoid of tenacity, and is extremely brittle.

It melts at a much lower temperature than that of boiling water, burns with a bright flame, and emits an odour ſomewhat reſembling that of ſpermaceti.

Water does not act upon it, neither does cold alcohol; but this laſt, when boiled, partially diſſolves it, and, upon cooling, depoſits the greater part; a ſmall portion, however, remains in ſolution, and may be precipitated by water.

Sulphuric ether, when heated, alſo diſſolves it; but, upon cooling, nearly the whole is depoſited.

Lixivium of potaſh, when boiled with the wax, forms a milky ſolution; but the chief part of the wax floats on the ſurface, in the ſtate of white flocculi, and appears to be converted into a ſoap of difficult ſolubility; it is no longer inflammable, and, with water, forms a turbid ſolution, from which, as well as from the ſolution in potaſh, the wax may be precipitated by acids.

Ammonia, when heated, alſo diſſolves a ſmall portion of the wax, and forms a ſolution very ſimilar to the former.

Nitric and muriatic acids do not ſeem to act upon the wax; the effects of ſulphuric acid have not been examined.

When the properties of this ſubſtance are compared with thoſe of bees-wax, a difference will be perceived; and, on the contrary, the moſt ſtriking analogy is evident, between the wax of lac and the myrtle wax which is obtained from the Myrica cerifera.

It greatly reſembles myrtle wax.

An account of the latter ſubſtance has been publiſhed by Dr. Boſtock, of Liverpool, in Nicholſon's Journal, with comparative Experiments and Obſervations on bees-wax, ſpermaceti, adipocire, and the cryſtalline matter of biliary calculi.*

The properties of the myrtle wax, as deſcribed in Dr. Boſtock's valuable paper, ſo perfectly coincide with thoſe which I have obſerved in the wax of lac, that I cannot but conſider them as almoſt the ſame ſubſtance; indeed I think they may be regarded as abſolutely identical, if ſome allowance be made for the ſlight modifications which have been produced by the different mode of their formation.

Component parts of lac.

From the preceding experiments and analyſes we find, that the varieties of lac conſiſt of the four ſubſtances which have been deſcribed, namely, extractive colouring matter, reſin, gluten, and a peculiar kind of wax. Reſin is the predominant ſubſtance; but this, as well as the other ingredients, is liable, in a certain degree, to variation in reſpect to quantity.

According to the analyſes which have been deſcribed, one hundred parts of each variety of lac yielded as follows:

Stick Lac.

Reſin - - - - -	68.
Colouring extract - - -	10.
Wax - - - - -	6.
Gluten - - - - -	5.50
Extraneous ſubſtances - -	6.50
	9.60.

Seed Lac.

Reſin - - - - -	88.50
Colouring extract - - -	2.50
Wax - - - - -	4.50
Gluten - - - - -	2.
	97.50.

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* Nicholſon's Journal for March, 1803, p. 129.

Shell Lac.

Reſin - - - -	90.90
Colouring extract - - -	0.50
Wax - - - -	4.
Gluten - - - - -	2.80
	98.20.

The proportions of the ſubſtances which compoſe the varieties of lac, muſt however be ſubject to very conſiderable variations; and we ought therefore only to conſider theſe analyſes in a general point of view. Hence we ſhould ſtate, that lac conſiſts principally of reſin, mixed with certain proportions of a peculiar kind of wax, of gluten, and of colouring extract.

The relative quantity of the two latter ingredients, very conſiderably effect the characters of the lacs; for inſtance, we may obſerve, that the glutinous ſubſtance, when preſent in ſhell lac in a more than uſual proportion, probably produces the defect obſerved in ſome kinds of ſealing wax, which, when heated and burned, become blackened by particles of coal; for the gluten affords much of this ſubſtance, and does not melt, like the reſin and wax. From what has been ſtated, therefore, lac may be denominated a *cero-reſin*, mixed with gluten and colouring extract.

§ III.

GENERAL REMARKS.

Lac has the characters of vegetables.

From the whole of the experiments which have been related, it appears that although lac is indiſputably the production of inſects, yet it poſſeſſes few of the characters of animal ſubſtances: and that the greater part of its aggregate properties, as well as of its component ingredients, are ſuch as more immediately appertain to vegetable bodies.

It is very uſeful;

Lac, or gum lac, as it is popularly but improperly called, is certainly a very uſeful ſubſtance; and the natives of India furniſh full proofs of this, by the many purpoſes to which they apply it.

for rings, beads;

According to Mr. Kerr, it is made by them into rings, beads, and other female ornaments.

ſealing wax, varniſhes;

When formed into ſealing wax, it is employed as a japan, and is likewiſe manufactured into different coloured varniſhes.

lakes, dying;

The colouring part is formed into lakes for painters: a ſort of Spaniſh wool for the ladies is alſo prepared with it: and, as a dying material, it is in very general uſe.

grindſtones, and poliſhers;

The reſinous part is even employed to form grindſtones, by melting it, and mixing with it about three parts of ſand. For making poliſhing grindſtones, the ſand is ſifted through fine muſlin; but thoſe which are employed by the lapidaries, are formed with powder of corundum, called by them Corune.*

the Hindû ink;

But, in addition to all the above uſes to which it is applied in India, as well as to thoſe which cauſe it to be in requeſt in Europe, Mr. Wilkin's Hindû ink occupies a conſpicuous place, not merely on account of its uſe as an ink, but becauſe it teaches us to prepare an aqueous ſolution of lac, which probably will be found of very extenſive utility.

water varniſh:

This ſolution of lac in water may be advantageouſly employed as a ſort of varniſh, which is equal in durability, and other qualities, to thoſe prepared with alcohol; whilſt, by the ſaving of this liquid, it is infinitely cheaper.

I do not mean however to aſſert that it will anſwer equally well in all caſes, but only that it may be employed in many. It will be found likewiſe of great uſe as a vehicle for colours; for, when dry, it is not eaſily affected by damp, or even by water.

The latter very uſeful. Inſtances.

With a ſolution of this kind, I have mixed various colours, ſuch as vermillion, fine lake, indigo, Pruſſian blue, ſap green, and gamboge; and it is remarkable, that although the two laſt are of a gummy nature, and the others had been previouſly mixed with gum, (being cakes of the patent water-colours,) yet, when dried upon paper, they could not be removed with a moiſtened ſponge, until the ſurface of the paper itſelf was rubbed off.

In many arts and manufactures, therefore, the ſolutions of lac may be found of much utility; for, like mucilage, they may be diluted with water, and yet, when dry, are little if at all affected by it.†

We

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* Phil. Tranſ. 1781, p. 380.

† The alkaline ſolutions of lac are evidently of a ſaponaceous nature, and, like other ſoaps, may be decompoſed by acids. The entire

Refins acted on by acids and alkalis.

We find, from the experiments on lac, that this ſubſtance is ſoluble in the alkalis, and in ſome of the acids. But this fact (conſidering that reſin is the principal ingredient of lac) is in oppoſition to the generally received opinion of chemiſts, namely, that acids and alkalis do not act upon reſinous bodies. Some experiments, however, which I have made on various reſins, gum-reſins, and balſams, fully eſtabliſh, that theſe ſubſtances are powerfully acted upon by the alkalis, and by ſome of the acids, ſo as to be completely diſſolved, and rendered ſoluble in water.

Field of inquiry.

It will be a very wide and curious field of inquiry, to diſcover what changes are thus produced in theſe bodies, eſpecially by nitric acid. Each ſubſtance muſt form the ſubject of a ſeparate inveſtigation; and there cannot be a doubt but that much will be learned reſpecting their nature and properties, which hitherto have been ſo little examined by chemiſts.

Utility of reſinous ſolutions.

The alkaline ſolutions of reſin may be found uſeful in ſome of the arts; for many colours, eſpecially thoſe which are metallic, when diſſolved in acids, may be precipitated, combined with reſin, by adding the former to the alkaline ſolutions of the latter. I have made ſome experiments of this kind with ſucceſs; and perhaps theſe proceſſes might prove uſeful to dyers and manufacturers of colours. It is probable alſo, that medicine may derive advantages from ſome of this extenſive ſeries of alkaline and acid ſolutions of the reſinous ſubſtances.

entire ſubſtance of lac is not however completely diſſolved, as appears from the turbidneſs of the liquors. Three of the four ingredients, namely, the reſin, the gluten, and the colouring extract, appear to be in perfect ſolution; whilſt the wax is only partially combined with the alkali, and forms that imperfectly ſoluble ſaponaceous compound which has been formerly mentioned, and which remains ſuſpended, and diſturbs the tranſparency of the ſolution.

From various circumſtances, it does not ſeem improbable, that the long ſought for, but hitherto undiſcovered vehicle employed by the celebrated painters of the Venetian School, may have been ſome kind of reſinous ſolution, prepared by means of borax, or by the alkalis.

A ſimple

VIII.

A ſimple and accurate Method of Surveying on Shore, with ſuch Inſtruments only as are in every one's Poſſeſſion. By Captain J. MORTLOCK. *From the Author.*

To Mr. NICHOLSON.

28, Surry Place, Kent Road,
7th January, 1805.

SIR,

Introductory letter.

ALMOST all our treatiſes on nautical ſurveying begin with the explanation and deſcription of what are called the neceſſary inſtruments, which are deſcribed ſo numerous, and the price ſo conſiderable, that very few can procure them: Thus diſcouraged, they abandon every idea of making plans of ſuch ports as they touch at, for want of what they conceive to be the neceſſary inſtruments.

To obviate this difficulty, and to render nautical ſurveying more general, I have, in the annexed paper, attempted to ſhew the mariner how to ſurvey any port or place he may touch at, with great accuracy, little trouble, and without any expence for inſtruments. Should you find this ſimple method deſerving of a place in your valuable Journal, I ſhall feel myſelf honoured by your inſerting it.

I am, Sir,
Your moſt obedient humble ſervant,
J. MORTLOCK.

Nautical ſurveying without inſtruments.

FIRST make an eye-ſketch of the place to be ſurveyed, as the annexed figure, numbering all the points, bays, rocks, ſhoals, &c. Chooſe two ſtations, as A and B (*Fig.* 1, *Plate V*), whence all the rocks, points, &c. may be ſeen from, and ſo ſituated from each other, that the bearings of the points, &c. as taken from A and B, ſhall interſect at angles at leaſt greater than 10 degrees, but the nearer 90 degrees the better.

Having choſen the ſtations, proceed to one of them as A, and place the paper intended to receive the plan horizontally before you, extended by pins, or otherwiſe, on a board ſecurely fixed, to prevent it ſhifting its poſition while the bearings are drawing.

Stick

Nautical ſurveying without inſtruments.

Stick a pin through the paper firm into the board, at the part meant to repreſent the ſtation A, and lay a ruler with a perfect ſtraight edge on the paper, touching the pin at A and pointing towards the ſtation B, and draw the line A B: in like manner draw lines from A towards all the points, rocks, bays, &c. numbering the lines as the points, rocks, bays, &c. are numbered in the eye-ſketch: Proceed next to the ſtation B, and place the board horizontally before you, ſo that the line A B ſhall point back towards A, and ſecure the board with the ſame precaution as at A, to prevent its ſhifting: then, in the line A B, ſtick a pin firm through the paper into the board, in that part meant to repreſent the ſtation B; from which point draw lines pointing towards the different points, rocks, &c. as was done from A, numbering them in like manner. Now, where the lines drawn from B interſect thoſe of the ſame number drawn from A, will be the place of the points, rocks, &c. to which the lines were directed to from the ſtations. Sketch in the ſhore between the points, &c. and the plan is completed.

The meridian-line may be found by compaſs, or more correctly, by drawing the line of the ſun's bearing from one of the ſtations, and taking his altitude at the ſame time. Then with the latitude, altitude, and declination, compute the azimuth, and lay it off to the left or right of the line of the ſun's bearing, according as the ſun was to the right or left of the meridian, and it will give the true north and ſouth, or meridian-line.

If the diſtance between any two points on the ſhore be meaſured, it will give you a ſcale for the plan; but it may often be found more convenient to meaſure off a baſe, as A C, from one of the ſtations, in a direction nearly perpendicular to the line A B; and let it be in length equal to ſome part of a geographical mile, as 380 feet $= \frac{1}{16}$, or 760 $= \frac{1}{8}$, or 1520 $= \frac{1}{4}$, or 3040 $= \frac{1}{2}$, or any part of a mile; then will the line A C be a ſcale to the plan.

I have ſuppoſed any common board and ruler to illuſtrate the ſimplicity of this method of ſurveying: but to ſuch as are provided with a drawing frame, it will be found convenient to extend the paper on; and if a ruler has ſights perpendicular to its edge, it will be found commodious, and require leſs trouble. I hope the eaſe and expedition with which the whole is performed, will induce ſea-faring people to amuſe themſelves by taking plans of the places they touch at: for it is by the improvement of geography that the dangers of navigation are diminiſhed, and, conſequently, the lives and property embarked in our ſhipping are leſs expoſed to danger.

J. MORTLOCK.

IX.

Notice of an Omiſſion in Accum's *Chemiſtry, of the direct Production of Nitric Acid. By* W. F. S.

To Mr. NICHOLSON.

SIR, *Lincoln's Inn, January* 7, 1805.

Nitre and nitric acid in Accum's chemiſtry.

HAVING in your Philoſophical Journal ſpoken very handſomely, and not undeſervingly ſo, of Mr. Accum's Treatiſe on Chemiſtry, it would not perhaps be improper in one of your enſuing Numbers, either to elucidate or correct a ſmall difficulty, or rather error, in that book. In deſcribing the mode of producing nitric acid, in the ſecond volume of Mr. A.'s work, p. 286, he puts as a principal ingredient *nitrate of potaſh:* Now, on referring to the mode of obtaining *nitrate of potaſh* in page 287, and following page, it is deſcribed as being produced by neutralizing the carbonate of potaſh with *nitric acid:* Now, Sir, it is pretty evident, that the means of preparing either nitric acid or nitrate of potaſh is not given, or rather it is ſtated by *implication,* as *incapable of being produced by art.*

Very reſpectfully your's,

W. F. C.

P. S. As this error can only be rectified by a ſubſequent edition, it would, with ſubmiſſion, be extremely beneficial to the holders of the preſent edition, that you ſhould introduce the emendation in your Journal.

X.

On Galvaniſm. By Mr. Charles Sylvester.

To Mr. NICHOLSON.

SIR,

Difficulties reſpecting the galvanic decompoſition of water; as to the diſtances of the wires.

YOU did me the honour of inſerting in your valuable Journal, ſome experiments tending to illuſtrate the theory of galvaniſm. I do not know who originally propoſed the idea of the combination of electricity with hydrogen (the truth of which my experiments were intended to eſtabliſh), though I am now bound to acknowledge the ingenuity and importance of the thought. Such an idea would perhaps never have been ſuggeſted, had it not been for the very paradoxical appearances attending the decompoſition of water by galvaniſm. The appearance of the hydrogen at ſo great a diſtance from the oxigen, both of which muſt have been produced from the ſame particle of water, was very ſatisfactorily accounted for by this conjecture. The continental philoſophers, and Dr, Gibbes of Bath, did not ſo well account for the phenomena by their hypotheſis, though it appeared ſo formidable, as to threaten the theory of modern chemiſtry with diſſolution.

Hypotheſis of Mr. Wilkinſon cenſured.

Another hypotheſis of the decompoſition of water, was given by Mr. Wilkinſon, in his Elements of Galvaniſm, and in your 36th Number. If we even allow Mr. W. the advantage of all his very gratuitous data in accounting for the decompoſition of water, the contradiction with which they abound, will totally render his hypotheſis invalid.

Remarks in detail on Mr. W.'s hypotheſis.

Mr. W. begins by ſuppoſing a particle of water analogous to the Leyden phial, which is the ſame thing as to ſuppoſe that water, a conductor of electricity, is compoſed of particles in themſelves non-conductors. Mr. W. is of opinion, that the decompoſition takes place in the middle of the liquid between the points of the wires. The way in which he ſuppoſes the ſeparation to be effected, is ſomething like the idea the ancients had of the ſolutions of metals in acids, *viz.* that the metal was ſplit into very minute particles (capable of being ſuſpended in the liquid) by the wedge-like particles of the acid. After the decompoſition is effected, he tells us that the capacity of the hydrogen is diminiſhed for electricity, while the capacity of oxigen is increaſed for the ſame ſubſtance; two facts for which I ſhould be glad to know Mr. W.'s authority. He now ſuppoſes the oxigen to be attracted by the poſitive wire, where it ſaturates itſelf with the electricity neceſſary to conſtitute its gaſeous form. The hydrogen, on the other hand, is attracted to the negative wire, to which it muſt give its exceſs before its elaſtic form can be effected.

Objection.

After all the labour Mr. W. has beſtowed to bring the hydrogen to one wire, and the oxigen to the other, he does not appear, in my opinion, to have ſucceeded; for the exceſs of electricity in the hydrogen would be given to the deficient oxigen, and the gaſſes would be given out in the middle of the liquid, and not at the ends of the wires.

Water not decompoſed in a very ſmall tube.

Mr. W. mentions the curious fact of water not being decompoſed in a very ſmall tube, and alſo, that no decompoſition takes place when the wires in the liquid are as much as eight inches diſtant from each other. I have long been in poſſeſſion of theſe facts: I have varied theſe experiments by uſing, inſtead of pure water, different ſolutions of ſalts, with a view to increaſe the conducting power of the liquid medium: I found a ſolution of carbonate of potaſh to anſwer very well: I placed the wires in the ends of a tube of more than a yard in length: The decompoſition went on very rapidly: I ſoon obſerved the poſitive wire coated with beautiful carbonate of copper, at the ſame time I obſerved a gaſeous ſubſtance diſengaged: I found it, by the teſt of lime-water, to be carbonic acid gas. I obſerved the diſengagement of this gas, after I had rendered the potaſh very cauſtic by lime.

Saline ſolutions are decompoſed.

Carbonate of potaſh.

Pure potaſh.

Common ſalt.

I afterwards uſed a tube of $\frac{1}{20}$th of an inch in diameter, and five feet long, into which I introduced a ſaturated ſolution of common ſalt. After the wires were introduced, and the communication made, I obſerved bubbles of hydrogen upon the negative wire, in about a minute after *.

I remain, Sir,

Your's, &c.

CHARLES SYLVESTER.

Sheffield, Jan. 20, 1805.

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* This corroborates the fact of the hydrogen paſſing inviſibly through the liquid.

XI.

Reply to the Animadverſions and Experiments of C. L. *on the Subject of the Horizontal Moon. By* Mr. EZEKIEL WALKER.

To Mr. NICHOLSON.

DEAR SIR,

WHAT can have induced your correſpondent C. L. to attempt to confute my paper relating to the horizontal moon, with ſo much invidiouſneſs, is beſt known to himſelf. For my own part, I believe that he is perſonally unknown to me, and that I am equally ſo to him.

Whether the teleſcope be properly urged againſt Mr. W.'s theory.

The firſt ſtep that this faſtidious writer takes to diſprove the truth of my poſition is a falſe one. He ſays, that "if Mr. Walker's poſition were true, the magnifying power of the ſame teleſcope would vary with its aperture." *

C. L. has been very unfortunate in mentioning the teleſcope, for the properties of that inſtrument will confute every argument which he has uſed againſt my theory, and ſhow that his ill conducted experiments, like an *ignis fatuus*, tended only to miſlead him.

The want of a ſtandard would prevent its being ſeen whether a leſs light gives a greater appearance to objects.

I ſhall drop this ſubject for the preſent, to examine his next aſſertion in the ſame paragraph. The apparent magnitude of the paper before the eye may become larger, when the candle is ſhaded with the hand, for any thing that C. L. knows to the contrary, as the increaſe is too ſmall to be perceived by our ſenſes; and even if it were ten times larger than it is, it could not be known, becauſe every other object before the eye would increaſe in apparent magnitude at the ſame time, and in the ſame ratio; and conſequently leave no ſtandard to compare the paper with. To elucidate this in a familiar way, permit me, Sir, to aſk how would C. L. determine the number of miles between Troſton in Suffolk, and Soho-Square in London, without ſome ſtandard meaſure, with which to compare that diſtance.

Strictures on C. L.'s experiments.

Then follow his "correct experiments of the ſame deſcription as mine."—My experiments were made to imitate the eye, upon a large ſcale. Let us ſee how C. L. has imitated nature in his experiments.

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* Philoſ. Journal, Vol. IX. p. 235.

Exp. 1. The object was too near the lens.

In his firſt experiment, "from the flame to the lens the diſtance was 49½ inches, and from the lens to the image it was 88 inches." Theſe numbers being reduced to the ſcale of the human eye, it will appear that the diſtance between the cryſtalline lens and the retina, is nearly twice as great as between the cryſtalline lens and the object; conſequently, if we take the diſtance between the cryſtalline and the outward ſurface of the cornea into the calculation, the object muſt nearly touch the eye. This, I think, may be called *a ſhort-ſighted experiment.*

The concluſion of C. L.'s paper contains a number of particulars, in which he *ſuppoſes* I have erred, and then modeſtly "ſubmits it to yourſelf and readers, whether I have acted conſiſtently with the rules of philoſophical inveſtigation," &c.

But as it will appear hereafter, that none of thoſe errors had any exiſtence, except in C. L.'s own mind, his concluſion can ſerve no other purpoſe than to ſhew the temper and diſpoſition of the writer.

A more particular account of the author's former experiments.

In my paper which was honoured with a place in the 9th vol. of this Journal, page 164, I only gave an abſtract of a ſeries of experiments, on which I founded my general concluſion; but it now appears neceſſary to give a more particular account of them, to ſhew that they are entitled to more credit than C. L. has thought fit to give them.

After having prepared my apparatus as deſcribed in my paper above-mentioned, with a long mould candle of ſix to the pound, placed in an inclined poſition, I began with meaſuring the focal image of the whole lens.

The length of this luminous picture upon the ſcreen, was determined by a pair of compaſſes, and a diagonal ſcale of inches. This meaſure was entered on page 1 of a book, prepared for that purpoſe.

The card No. 1. was then placed before the lens; the luminous picture meaſured in the ſame manner as before, was entered on page 2 of the ſame book.

The meaſure of the picture given by the aperture, No. 2. was entered on the 3d page. And the meaſure of that given by No. 3. was noted down on the 4th page.

As ſoon as theſe four experiments were finiſhed, I began again with the whole lens, and entered a ſecond meaſure on page 1. under the firſt, and continued till I had obtained two meaſures on each page: and in this manner, I continued my experiments until I had obtained five or ſix meaſures to each aperture. This mode gave me an opportunity of ſeeing how much the flame of the candle altered in its length, during the time that I was making the experiments, which alteration was too ſmall to be regarded. The meaſures on each page being added up, and divided by the number of them, gave me thoſe numbers which were inſerted in my paper.

But I did not ſtop here, for ſets of experiments were repeated in the ſame manner, on ſeveral evenings, to aſcertain the fact more clearly. But to come more immediately to the point; when the ſun or moon was uſed inſtead of a candle, the ſame reſult was obtained, viz. the greateſt aperture gave the greateſt luminous picture.

Principles of optics brought in ſupport of the author's theory.

The truth of this property in optics, however, does not wholly reſt on my experiments, the ſame concluſion may be derived from other principles.

The pencils of light that enter the eye are ſmaller, the ſmaller the aperture of the object lens;

It is a well known property of the teleſcope, that "as the aperture is contracted, the ſlender pencils or cylinders of rays that emerge from the eye-glaſs into the eye, are alſo contracted in the ſame proportion." *

The magnifying power, of the object glaſs of a teleſcope is not increaſed by increaſing its aperture; but by increaſing its aperture, you increaſe the magnitude of the pencil of light in its focus: for as the eye-glaſs remains the ſame, the increaſe in magnitude of the emerging pencil of rays, muſt depend upon an increaſe in magnitude of the pencil in the focus of the object glaſs. Conſequently, when the moon is viewed, if the aperture of the object glaſs be increaſed, the picture of the moon within the teleſcope, will be increaſed in the ſame ratio; but without increaſing the magnifying power of the inſtrument.

—whence it is inferred that the moon's picture will be equally changed in ſize.

For the magnifying power is = the diameter of the aperture of the object glaſs divided by the diameter of the emerging pencil.

Demonſtration.

Let A = the diameter of the aperture of the object glaſs, B = the diameter of the emerging pencil, C = the focal diſtance of the object glaſs, and D the focal diſtance of the eye-glaſs., Then $\frac{C}{D}$ = the magnifying power.

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* Elementary Parts of Smith's Optics, page 93.

But C : D : : A : B. (Smith's Optics, p. 93.) And by diviſion $\frac{C}{D}=\frac{A}{B}$. Therefore the enlargement of the picture of an object within the teleſcope, does not increaſe its magnifying power.

Concluſion.

Hence theſe properties of the teleſcope prove to a demonſtration, that the concluſion which I have drawn from my experiments is a law of viſion; and this law ſhows us, that no concavity of the ſky; no terreſtrial perſpective, nor even *the painting of the waves under Black-Friar's Bridge,* can explain the phenomenon in queſtion *. The cauſe lies wholly within the eye.

Reſpecting controverſy.

It is a moſt unpleaſant taſk to enter into controverſy, but I cannot ſee how I could avoid it in this inſtance. It appears to me, Sir, that you inſerted C. L.'s paper for me to anſwer, and I have complied; hoping, however, that this is the laſt time of my having occaſion to uſe my pen, againſt an anonymous writer. Common juſtice ſeems to demand the names of thoſe who undertake the examination of ſuch papers as are owned by their reſpective authors.

I am, Dear Sir,
Your moſt obedient ſervant,
EZ. WALKER.

Lynn, Jan. 18, 1805.

XII.

Deſcription of a ſimple Inſtrument for making correct Drawings from Nature. By T. C. B.

To Mr. NICHOLSON.

SIR, *January* 22, 1805.

THE deſcription of two inſtruments for facilitating landſcape drawing from nature given in the firſt volume, page 281, of your Journal, has ſuggeſted to me the conſtruction of

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* Philoſophical Journal, vol. IX. page 237.

another,

Inſtrument for making correct drawings from nature.

another, which ſeems to unite the accuracy of the firſt of thoſe inſtruments with the ſimplicity and portability of the ſecond. As I conceive it may be of ſome ſervice to thoſe who are in the habit of ſketching landſcape, I ſend you a deſcription of one which I have had made, that you may, if you think proper, publiſh it in your Journal. Let A B, *Fig.* II. *Plate* 5. be a flat rule, ſuppoſe twelve inches long, having at its extremities two arms B D and A F turning upon a joint at B and A; and in each arm a circular joint at C and G: let the length of each arm B D and A F be ten inches: a handle to fix on E, and a thread paſſing through two holes equidiſtant from the handle, making any length, according to the angle under which the view may be beſt ſeen.

To uſe the inſtrument, take the end of the thread in the mouth, and hold the inſtrument upright before the eye, then move either or both of the arms till the points D and F are brought in a line between the eye and any point in the landſcape you may wiſh to delineate; lay the inſtrument upon the drawing paper, and you will have the true ſituation of ſuch part of the ſubject.—Proceed in like manner, taking care always to keep the baſe line in the ſame place, till you get as many points as you require, by which means any landſcape or building may be drawn very expeditiouſly, and with a great degree of accuracy.

To make the inſtrument as portable as poſſible, there is a joint in A B, which the handle covers, and the pieces D G and C F are made of thin braſs, to fold into the pieces A C and B G; ſo that the inſtrument, when folded up, need occupy no more room in the pocket than a ſmall ſpectacle caſe.

It is perhaps unneceſſary to add that this inſtrument may be uſed for the purpoſe of copying, and anſwers the purpoſe of a triangular or quadrangular compaſs.

I am, Sir,

Your moſt obedient and humble ſervant,

T. C. B.

If the rough incloſed ſketch of the inſtrument is not ſufficiently intelligible, Mr. Nicholſon may ſee one at Mr. Fraser's, Optician, in Bond-Street.

XIII.

Obſervations on Baſalt, and on the Tranſition from the vitreous to the ſtony Texture, which occurs in the gradual Refrigeration of melted Baſalt; with ſome geological Remarks. In a Letter from GREGORY WATT, *Eſq. to the Right Hon.* CHARLES GREVILLE, *V. P. R. S. From the Philoſophical Tranſactions for* 1804, *p.* 279.

SIR, *Soho, April* 1804.

THE important geological conſequences that ſeem deducible from the changes of texture developed by Sir James Hall's very judicious experiments on the regulated cooling of melted baſalt, induced me to attempt a repetition of them, ſome time after the publication of his intereſting and ingenious paper.* I believe that formerly I had the honour of ſhowing you ſome of the reſults of my imperfect and diminutive experiments, which only ſerved to afford additional proofs of the tranſition from the vitreous to the ſtony texture, which takes place in the gradual refrigeration of glaſs. Circumſtances have prevented my reſuming theſe inveſtigations, till it lately occurred to me that ſomething might be learned, by expoſing to the action of heat, a much larger maſs of baſaltic matter than, as far as I am informed, had ever at one time been ſubjected to experiment.

Sir James Hall's experiments on the ſlow congelation of fuſed baſalt.

One of the common reverberatory furnaces uſed in iron founderies for the fuſion of pig-iron, was ſtrongly heated by a fire maintained for ſeveral hours. About ſeven hundred weight of amorphous baſalt, here called Rowley Rag, was broken into ſmall pieces, and depoſited gradually on the elevated part of the interior of the furnace, between the fire and the chimney, from whence, as it melted, it flowed into the deeper part, in which, in ordinary operations, the melted iron is collected. It was obſerved by the perſons attending, that it did not require half the quantity of fuel to fuſe the baſalt, that would have been neceſſary to melt an equal weight of pig-iron. When the whole was melted, it formed a liquid glaſs, rather

The experiment repeated on 7 cwt. of rowley rag.

It was eaſily fuſed, and formed perfect glaſs when quickly cooled.

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* Publiſhed in the Tranſactions of the Royal Society of Edinburgh, Vol. V. and in our Journal, Vol. V. quarto ſeries.

tenacious, from which a large ladle-full was taken, which, on being allowed to cool, retained the characters of perfect glaſs. The fire was maintained, though with gradual diminution, for more than ſix hours; after which time, the draught of the chimney was intercepted, the ſurface of the glaſs was covered with heated ſand, and the furnace was filled with coals, which were conſumed very ſlowly. It was eight days before the maſs in the furnace was ſufficiently cool to be extracted, and even then it retained conſiderable internal heat.

Slow congelation.

Appearance of the maſs.

The form of the maſs, being given by the bottom of the furnace, was conſiderably irregular, approaching to the ſhape of a wedge whoſe lower angles were rounded. It was nearly three feet and a half long, two feet and a half wide, about four inches thick at one end, and above eighteen inches at the other. From this diverſity of thickneſs, and from the unequal action of the heat of the furnace, too great an irregularity had prevailed in the refrigeration of the glaſs, to permit its attainment of a homogeneous texture. Theſe circumſtances might probably have been counteracted by better deviſed precautions; but the inequality of the product is not to be regretted, ſince it has fortuitouſly diſcloſed ſome very ſingular peculiarities, in the arrangement of bodies paſſing from a vitreous to a ſtony ſtate, which might have remained unobſerved, if the deſired homogeneity of the reſult had been obtained. I ſhall now endeavour to deſcribe the various products of this operation; and I ſhall alſo ſubmit to your conſideration, ſome remarks which appear to me to ariſe naturally from the phenomena I have obſerved; premiſing that, except where my opinions are ſupported by the unequivocal demonſtration of facts, I offer them with the utmoſt deference to the deciſion of more experienced and judicious mineralogiſts and geologiſts.

External characters, &c. of rowley rag.

It may be proper to give a conciſe deſcription of rowley rag itſelf before I conſider the products which it yields by igneous fuſion. This ſpecies of baſalt is fine-grained, of a confuſed cryſtallized texture; its fracture uneven in ſmall pieces, conchoidal in large pieces. Its hardneſs ſuperior to common glaſs but inferior to feldſpar. Its tenacity conſiderable. Its action on the magnetic needle ſtrong, but without ſigns of polarity. Its ſpecific gravity, according to my trials, 2.868. Its general colour iron gray, approaching to black. It is opaque; and it reflects light from a number of brilliant points, ſome of which ſeems to be feldſpar, and the others hornblende.*

Its glaſs.

1ſt. This ſubſtance is eaſily fuſed into glaſs, whoſe texture is completely vitreous, with few air-bubbles. Its fracture undulated conchoidal. Its hardneſs ſuperior to feldſpar, but inferior to quartz. It poſſeſſes ſcarcely any action on the magnetic needle. Its colour is black: it is nearly opaque, being tranſlucent only in very thin fragments. Its ſpecific gravity appears to be 2.749.

Firſt appearance of arrangement. Small globules appear in the maſs.

2d. The tendency towards arrangement in the particles of the fluid glaſs, is firſt developed by the formation of minute globules, which are generally nearly ſpherical, but ſometimes elongated, and which are thickly diſſeminated through the maſs. The colour of theſe globules is conſiderably lighter than that of the glaſs; they are commonly grayiſh-brown, ſometimes inclining to chocolate brown, and, when they have been formed near the interior ſurface of the cavities in the glaſs, they project, and reſemble a cluſter of ſmall ſeeds. Their diameter rarely exceeds a line, and ſeldom attains that ſize, as, in general, they are ſo near to one another, that their ſurfaces touch before they can acquire conſiderable magnitude. In the proceſs of cooling, they adapt their form to their confined ſituation, fill up every interſtice, and finally preſent a

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Analyſis of rowley rag by Dr. Withering.

* "The ragſtone has been accurately analyſed by Dr. Withering, who found that 1000 parts of it contained 475 parts of ſiliceous earth, 325 argillaceous earth, and 200 calx of iron; but this iron ſeems to me to be in a very ſmall degree of calcination, from the dark blue colour of the ſtone, from the ruſty colour it aſſumes on being expoſed to a farther ſtate of calcination by air and water, and from the magnetic property of the mountains, which, as Dr. Plot obſerved, turned the needle 6° from its proper direction. This magnetic property has ſince been obſerved in ſeveral baſaltic mountains, particularly in the Giant's Cauſeway in Ireland, and very remarkably in a baſaltic columnar mountain called Compaſs Hill, in the iſland Cannay, one of the Hebrides, deſcribed by George Dempſter, Eſq. in the Tranſactions of the Society of Antiquaries in Scotland, Vol. I." See Mineralogy of the South-weſt part of Staffordſhire, by James Keir, Eſq. F. R. S. publiſhed in Shaw's Hiſtory of Staffordſhire, Vol. I.

Mr. Kirwan ſtates the ſpecific gravity of rowley rag, which he calls *ferrilite*, at 2.748; and aſſigns its melting point at 98° of Wedgwood's pyrometer.

homogeneous body, wholly unlike glaſs, and equally unlike the parent baſalt. When the union of the little globules has been imperfectly effected, the fracture of the maſs indicates its ſtructure, by numerous minute conchoidal fractures, which diſplay the form of each globule. But, if the arrangement has extended a little farther, all theſe ſubdiviſions are entirely loſt; the maſs becomes perfectly compact, has an even or a flat conchoidal fracture, is nearly of the ſame hardneſs as the glaſs, is commonly of a chocolate colour, graduating into a browniſh-black, and the intenſity of the colour increaſes in proportion to the degree to which the arrangement has extended. Its aſpect is rather greaſy; and it much reſembles ſome varieties of jaſper in the compactneſs of its texture, and in its opacity. Its magnetic action is extremely feeble. Its ſpecific gravity appears to be 2.938.

It reſembles jaſper, and has little magnetic action.

More advanced appearance of the ſtony texture.

3d. If the maſs were now rapidly cooled, it is obvious that the reſult would be the ſubſtance I have juſt deſcribed; but, if the temperature adapted to the farther arrangement of its particles be continued, another change is immediately commenced, by the progreſs of which it acquires a more ſtony texture, much greater tenacity, and its colour deepens as theſe changes advance, till it becomes abſolutely black. Sometimes this alteration is effected by a gradual tranſition, the limits of which cannot be aſſigned, but more generally by the formation of ſecondary ſpheroids, in the heart of the compact jaſpedeous ſubſtance. Theſe ſperoids differ eſſentially from thoſe firſt deſcribed; the centres of their formation are more remote from each other, and their magnitude is proportionably greater, ſometimes extending to a diameter of two inches, and ſeeming only to be limited by contact with the peripheries of other ſperoids. They are radiated, with diſtinct fibres; ſometimes the fibres reſemble thoſe of brown hæmatites, and ſometimes they are faſciculated irregularly, ſo as to be very ſimilar in appearance to the argillaceous iron ores rendered priſmatic by torrefaction. They are generally well defined, and eaſily ſeparable from the maſs they are engaged in; and often the fibres divide at equal diſtances from the centre, ſo as to detach portions of the ſperoid in concentric coats. The tranſverſe fracture of the fibres is compact and fine grained; the colour black; and the hardneſs ſomewhat inferior to that of the baſaltic glaſs. When

Darker colour.

Large ſpheroids of radiated texture.

two of the ſpheroids come into contact by mutual enlargement, no intermixture of their fibres ſeem to take place; they appear equally impenetrable, and, as neither can penetrate, both are compreſſed, and their limits are defined by a plane, at which a ſeparation readily takes place, and each of the ſides is inveſted with a ruſty colour. When ſeveral ſpheroids come in contact on the ſame level, they are formed by mutual preſſure into pretty regular priſms, whoſe diviſion is perfectly defined; and when, a ſpheroid is ſurrounded on all ſides by others, it is compreſſed into an irregular polyhedron.

Contact with ſpheroids produces polyhedrons.

Third arrangement. Compact ſtony texture, great tenacity, opake, conſiderable magnetic action.

4th. The tranſition from this fibrous ſtate to a different arrangement, ſeems to be very rapid; for the centre of moſt of the ſperoids becomes compact, before they attain the diameter of half an inch. As the fibrous ſtructure propagates itſelf by radiating into the unarranged maſs, the compact nucleus which ſupplies its place gradually extends, till it finally attains the limits of the ſpheroids; and the ſame arrangement pervades the matter comprehended between them. The maſs has now aſſumed a compact ſtony texture, and poſſeſſes great tenacity. Its hardneſs is ſomewhat inferior to that of the glaſs from which it was formed. Its action on the magnetic needle is very conſiderable. Its ſpecific gravity is 2.938. Its colour is black, inclining to ſteel gray: it is abſolutely opaque, and only reflects light from a few minute points. Though the diviſions between the ſpheroids are rendered imperceptible to the eye, they are not obliterated, and their ruſty ſurfaces are often diſcloſed by an attempt to fracture the maſs.

Granular texture.

Cryſtalline laminæ.

Polarity.

Increaſed denſity.

5th. A continuation of the temperature favourable to arrangement, ſpeedily induces another change. The texture of the maſs becomes more granular, its colour rather more gray, and the brilliant points larger and more numerous: nor is it long before theſe brilliant molecules arrange themſelves into regular forms; and, finally, the whole maſs becomes pervaded by thin cryſtalline laminæ, which interſect it in every direction, and form projecting cryſtals in the cavities. The hardneſs of the baſis ſeems to continue nearly the ſame; but the aggregate action of the baſis, and of the imbedded cryſtals, on the magnetic needle, is prodigiouſly increaſed. It appears to poſſeſs ſome polarity; and minute fragments are ſuſpended by a magnet. Its ſpecific gravity is ſomewhat increaſed, as it is now 2.949. The cryſtals contained in it, when examined by a microſcope, appear to be faſciculi of ſlender priſms, nearly rectangular, terminated by planes perpendicular to the axis; they are extremely brilliant; their colour is greeniſh-black; they are harder than glaſs, and fuſible at the blow-pipe; they are ſuſpended by the action of a magnet. They are arranged nearly ſide by ſide, but not accumulated in thickneſs, ſo that they preſent the appearance of broad thin laminæ; they croſs one another at all angles, but always on nearly the ſame plane; and the lamina thus formed is often three or four lines long, and from a line to a line and a half broad, but extremely thin.*

It ſeems obvious, that an equalized temperature would have rendered the whole ſimilar to the ſubſtance laſt deſcribed; and it may be fairly inferred, that by a continuance of heat, the minute cryſtals would have been augmented in their dimenſions, by the acceſſion of molecules ſtill engaged in the baſis, or by the union of ſeveral cryſtals, till they acquired ſufficient magnitude for their nature to be abſolutely determined by the uſual modes of inveſtigation. It is probable, however, if ſuch precautions had been taken as might have ſecured this degree of perfection in the ulterior reſult, that the maſs would only have exhibited an uniform aſpect, and that the intereſting initial phenomena would not have been diſcovered.†

There

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* It may be obſerved, that the cavities which exiſted in the glaſs are not obliterated during the ſubſequent proceſſes, though their interior ſurfaces undergo ſome change. The minute globules firſt formed often become prominent, and project into the cavities. Theſe minute points are ſoon obliterated by the large curves of the fibrous ſpheroids, which give a mamellated form to the interiors of the cavities; and, when the cryſtals are generated in the maſs, they ſhoot into ſome of the cavities, and line them with their brilliant laminæ.

† In this and the ſucceeding paragraphs, the word molecule is uſed in the ſenſe aſſigned to it by Hauy and Dolomieu, and is underſtood to repreſent the peculiar ſolids, of definite compoſition and invariable form, the accumulation of which, forms the cryſtals of mineral ſubſtances. Such molecules, preſerving their form and their eſſential characteriſtics, may be extracted from moſt cryſtals by mechanical diviſion, and may be ſubdivided as far as our ſenſes can recogniſe them. Though we cannot by mechanical means directly divide them into their elementary particles, we are enabled to

Explanation of the formation of the globules and ſpheroids; from the emiſſion of heat during congelation.

There are ſome conſiderations which appear to offer a partial explanation of the formation of the globules, and of the radiated ſpheroids. It is well aſcertained that heat is emitted by all bodies, in their change from a gaſeous to a fluid ſtate, and alſo in their change from a fluid to a ſolid ſtate. It is reaſonable to ſuppoſe, that heat may alſo be emitted in thoſe changes of arrangement which affect the internal texture of a body, after it has attained an apparently ſolid ſtate. That a ſucceſſion of ſuch changes does actually take place, appears to me demonſtrated by the appearances I have deſcribed, and by the increaſe of ſpecific gravity, which ſeems to keep pace with the internal changes of the ſubſtance. It would appear, that theſe changes are cauſed by a gradual diminution of temperature, which permits certain laws to induce peculiar arrangements among the particles of the glaſs. When ſeveral of theſe particles enter into this new bond of aſſociation, they muſt form a minute point, from which heat muſt iſſue in every direction. That heat will gradually propagate itſelf, till the temperature of the glaſs is equalized; and then the recurrence of the circumſtances which induced the firſt particles to arrange, will cauſe other particles to arrange alſo, which the attraction of aggregation will diſpoſe round the point firſt formed. A ſecond emiſſion of heat in every direction will take place; the temperature will again be equalized; and again another concentric coat of arranged particles will apply itſelf to the little globule. But, at the time when the central point of this globule was formed, the equality of temperature, in the maſs of glaſs, would probably cauſe a number of ſimilar points to be generated. The formation of each muſt proceed in a ſimilar manner to what I have deſcribed, till their ſurfaces touch, and all the glaſs be converted into the ſame ſubſtance.

General connection between the radiated ſtructure and formation by concentric coats.

Theſe globules are therefore formed of concentric coats; but they are alſo radiated. Every one muſt have remarked

to effect this by chemical ſolution, the only power to which their aggregation yields. It will be evident, from the obſervations that follow, that I am inclined to adopt the ingenious idea of Dolomieu, that many apparently homogeneous rocks are compounds of the minute molecules of ſeveral ſpecies of minerals; and that, where a ſuitable opportunity is given, theſe will develope themſelves by the formation of their peculiar cryſtals.

the connexion that almoſt uniformly exiſts, between the radiated ſtructure and the formation by concentric coats. There are few radiated ſubſtances which are not diviſible into concentric fragments; and as few concentric arrangements which are not radiated. Of the firſt, it may be ſufficient to mention hæmatites; of the ſecond, calcareous ſtalactites. The tendency to this union of ſtructure, may perhaps be produced by the radiation of the emitted heat, or moiſture, if the ſolution be aqueous; and the diviſions of the coats will naturally take place at thoſe pauſes in the accumulation of particles, which the momentary emiſſion of heat neceſſarily induced.

Formation of the larger ſpheroids explained.

If this be allowed to explain the formation of the firſt ſeries of globules which conſolidate into the jaſpideous ſubſtance, it will alſo explain the formation of the larger and more diſtinctly radiated ſpheroids, which have been already ſtated to be very eaſily diviſible into concentric fragments. They probably were alſo formed round a central point, by the accumulation of thin coats; and the tendency to radiation, which ſeems almoſt inſeparable from this ſtructure, was perhaps aided by the arrangement induced by the emiſſion of heat from every part of the ſurface of the ſpheroids. This mode of formation has the advantage of explaining their impenetrability. Had they been generated by radii diverging from a centre, their compactneſs muſt have diminiſhed as their diameter increaſed; but, in the ſtructure which I have ſuppoſed, each coat is compoſed of particles ſolidly arranged in immediate contact with each other, leaving no ſpaces for penetration. The ſame progreſs is rigidly obſerved in the extenſion of the compact nucleus, which always occupies the centre of the radiated ſpheroids, and finally extends to their peripheries. It obſerves the concentric diviſions of the radiated part with the greateſt preciſion; and the line of their ſeparation is always perfectly defined. But the ſtate of aggregation into which the ſubſtance has now entered, is ſo perfect as to overcome the operation of the cauſes which formerly induced the fibrous ſtructure, and the maſs remains compact. The only change that the ſubſtance afterwards undergoes, conſiſts in the gradual accumulation of the cryſtalline molecules, and their arrangement, by their individual polarity, into regular ſolids. This depends on very different laws from thoſe which conſolidated the fluid glaſs, and aggregated its particles into a compact uniform ſtone.*

Other conſiderations. Remarkable circumſtance, that theſe changes ſucceed after fluidity may be ſuppoſed to have ceaſed, &c.

The appearances that I have endeavoured to deſcribe, ſeem deſerving of conſideration in ſeveral points of view. Few things can be more at variance with commonly received opinion, than the diverſified ſucceſſion of changes of ſtructure which this glaſs exhibits in its paſſage to a cryſtallized ſtate. The generation of the globules which unite to form the jaſpideous ſubſtance, is what we might be prepared to expect, by obſerving the cooling of a common iron furnace ſlag. But it appears not very obvious to common apprehenſion, that the ſpecies of arrangement requiſite to form this intermediary ſubſtance, could be compatible with any fluidity permitting farther motion of the molecules of the maſs; yet, immediately after the completion of this arrangement, they receive a new diſpoſition, and the radiated fibrous ſtructure commences. Sometimes this pervades even the unaltered glaſs; but I preſume this only to happen where the minute globules firſt formed were ſcattered ſo far aſunder, that their centres became fibrous, before their peripheries came into contact. This view of the ſubject is juſtified by the analogous operation of the formation of cryſtals, ſimilar to thoſe deſcribed, in the heart of the radiated ſperoids, while their exteriors ſtill retained the fibrous texture.

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* The caſe is conſiderably different, where cryſtals poſſeſſing regular forms are generated in glaſs. The molecules of which they are formed, have doubtleſs been only ſuſpended in the vitreous medium; and their union is determined by cryſtalline polarity, which appears to me perfectly diſtinct from the ſimple aggregation which changes a fluid into a ſolid, whether it be homogeneous or compound, which affects the internal arrangement of thoſe bodies, but which never can ſeparate their components into diſtinct maſſes, or form them into regular ſolids. Every molecule, at the moment of its formation, muſt neceſſarily be endowed with all the properties it afterwards poſſeſſes. The ſuſpenſion of ſuch molecules in a fluid medium, though it may conceal, cannot alter thoſe properties; and the union of ſuch molecules, to form a regular ſolid, in no reſpect alters their individual or aggregate qualities. Whether heat be evolved at the moment of this union, is a queſtion not eaſily ſolved; as the cryſtallizations with which we are familiar are from chemical ſolutions, in which ſome of the molecules are generated by the ſeparation of a combined ſubſtance, at the moment when others are united by cryſtalline polarity.

—Review of the facts under this point of view.

If it be conſidered as extraordinary, that a change ſhould be effected, converting an apparently ſolid and homogeneous maſs into an accumulation of radiated ſpheroids, and that theſe radii ſhould loſe their fibrous ſtructure, and aſſume the texture, aſpect, and tenacity, of a compact, hard, and homogeneous ſtone, it is certainly much more extraordinary, that this ſtone ſhould permit farther arrangement to proceed, and ſhould enable the cryſtalline molecules which it contains in a ſtate of confuſed aggregation, to arrange themſelves, and to form cryſtals which, although minute, are equal in the perfection of their forms, and in the brilliancy of their natural poliſh, to the moſt precious products of cryſtallization. It is alſo well deſerving of obſervation, by how regular a march the magnetic influence of the ſubſtance keeps pace with the perfection of its arrangement, till it becomes ſo powerful, that fragments of the regenerated ſtone are ſuſpended by the attraction of a magnet.

The act of cryſtallization is incompatible with ſolution or fluid combination.

It has been moſt juſtly remarked by Mr. Smithſon, that ſolution, far from being neceſſary to cryſtallization, effectually prevents its commencement; for, while ſolution ſubſiſts, cryſtallization cannot take place. It may remain a queſtion, whether previous ſolution be eſſential, as a preparatory means of obtaining, by ſubſequent evaporation, or cooling, the ſmall parts of bodies diſengaged, ſo that they may unite to form regular cryſtals. If by ſolution be only meant, that ſimple action of heat, or water, which merely counteracts the force of aggregation, and relieves the molecules from their bonds of union with each other, it certainly is a requiſite; but if by ſolution be meant, that action of affinities by which not only the force of aggregation is overcome, but the combinations which conſtitute the molecules are deſtroyed, it obviouſly is not only unneceſſary but prejudicial to cryſtallization; as a new ſet of molecules muſt be formed, by a new combination of the elementary particles, before the formation of regular bodies can commence. The ſuſpenſion of the molecules ready to cryſtallize, may be correctly ſaid to be merely mechanical. Though the mechanical action of trituration can never be expected to reſolve even the moſt eaſily diviſible body in its molecules, becauſe the

Suſpenſion is neceſſary; but this may be called mechanical.

the fractures will be at leaſt as frequently acroſs the natural joints as in their direction, yet, even by this rude method, ſome perfect molecules may be diſſengaged; for we find, that water paſſing over large ſurfaces of ſiliceous ſand, finds ſome molecules of ſilex in the ſtate proper for aggregation, and even for cryſtallization. Mechanical ſuſpenſion in a fluid medium, of ſuch denſity that the cryſtalline polarity may be enabled to counteract the power of gravity, is with juſtice conſidered by Mr. Smithſon the only requiſite for the formation of cryſtals.* The circumſtances I have detailed, appear to me an additional confirmation of this remark, and perhaps go ſtill farther, by ſhowing that even the fluidity (in the common ſenſe of the word) of the ſuſpending medium is not an indiſpenſable condition. For it appears impoſſible to annex the idea of fluidity to the union of the minute globules which form the jaſpideous ſubſtance, ſtill leſs to that ſubſtance when formed, and ſtill leſs to thoſe ſpheroids whoſe obſtinate impenetrability is ſo ſtrongly defined. And if, by any power of imagination, theſe can be ſuppoſed to be fluid at the time they retain this conformation, how can it be ſuppoſed that the compact hard tenacious ſtone into which they are changed could retain theſe characters in a fluid ſtate? Yet the ſubſequent formation of cryſtals proves, that either all theſe contradictions muſt be, or that the particles of bodies apparently ſolid muſt be capable of ſome internal motion, enabling them to arrange themſelves according to polarity, while they are ſolid and fixed, as far as they have reference to the ordinary characters of fluidity.

Even fluidity does not ſeem an indiſpenſable condition.

Inſtances even more remarkable have very long been known and authenticated, though perhaps they have not been generally regarded with the attention then deſerve. Glaſs veſſels are well known to be convertible into Reaumur's porcelain, by the internal arrangement of their particles, without loſing their external form, and conſequently at a temperature very much below that requiſite for their fuſion, The change of glaſs into Reaumur's porcelain, does not ariſe from an evaporation of the alkali, as has been alledged, but from a regular arrange-

Inſtances of the internal motions and arrangements of particles of bodies at temperatures below fuſion.

Reaumur's porcelain fully deſcribed;

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* See a chemical Analyſis of ſome Calamines, by James Smithſon, Eſq. Philoſophical Tranſactions for 1803, page 27. See alſo Dolomieu, *Journal des Mines*, No. 22, page 56.

ment of the molecules of the glaſs. It commences by the formation of fibres perpendicular to the ſurface of the glaſs, and penetrating into it. At nearly the ſame time, ſmall radiated globules are formed in the interior of the glaſs, and the union of theſe with the fibres, by their mutual increaſe, forms the whole into a new ſubſtance; and, if the requiſite temperature be longer maintained, the fibres diſappear, and the whole becomes fine-grained, and almoſt compact. This ſubſtance, from the improved ſtate of its aggregation, is much ſtronger and more tenacious than before, and is not fuſible at a heat ſufficient to ſuſe the glaſs it was formed from; but, if that aggregation be once deſtroyed, the glaſs reſulting from its fuſion is equally fuſible with the original glaſs; and a repetition of the proceſs will again form Reaumur's porcelain, which may be again fuſed, and ſo on repeatedly, for the quantity of alkali evaporated during the operation is extremely ſmall. The hardneſs and brittleneſs of metals rapidly cooled, contraſted with the ſoftneſs and tenacity reſulting from their gradual refrigeration, are all analagous inſtances; and all the proceſſes in which annealing is employed, and more remarkably the tempering of ſteel, the proofs of the internal motions and arrangements of the particles of matter, at temperatures very much below the heat is requiſite for their fluidity.

hardening; tempering; annealing:

Cryſtallization of a maſs of the ſame nature throughout;

Whatever doubts may ariſe reſpecting the formation of the cryſtals, there ſeems no reaſon to ſuppoſe that their gradual increaſe would ceaſe, till all the molecules belonging to that ſpecies were exhauſted, if the temperature favourable to their generation was continued. If the maſs was entirely compoſed of one ſpecies of molecules, it would be reſolved into an aggregation of cryſtals of the ſame ſubſtance; and probably by a ſtill farther continuation of the proceſs of arrangement, into one cryſtal, which, though it might not poſſeſs a regular external form, would be perfect in its internal ſtructure.

of particles differing from each other.

But, if the maſs contains two diſtinct ſpecies of molecules, different reſults muſt take place, which will be modified by the proportional quantities of the components. As it has been demonſtrated by Berthollet, that the attraction of maſſes of matter are relatively as their quantities, it follows, that unleſs a very potent counteracting cauſe be exerted, the moſt abundant ingredient in the mixture will be the firſt to cryſtallize.

Generally, the moſt abundant ingredient will cryſtallize firſt.

But this cryſtallization will not comprehend the whole of its molecules; for, after a certain quantity of them are arranged, the proportions of the remaining fluid are altered; that ingredient which was before the leaſt, may now be equal, or even greateſt, and it will exerciſe its attraction. As the firſt cryſtallization, by ſubtracting a large portion of the fluid particles, muſt have obliged the molecules of the leſs abundant ſubſtance to approach each other very cloſely, they may be able to collect themſelves entirely in their firſt attempt to cryſtallize, or they may form alternate cryſtallizations with the remaining unarranged molecules of the more abundant ſubſtance. However various the ſpecies of molecules may be, they will be regulated by analogous laws, and only ſerve to diverſify the generated ſubſtances.

Diverſities.

It by no means follows, that the cryſtals afterwards found to be moſt infuſible would be firſt generated. Their formation does not altogether depend on their greater or leſs fuſibility, but on the relative ſtrength of the attraction which unites them to the matter they are immerſed in, and of the polarity which invites them to cryſtallize. In all cryſtallization from compound fluids, the order in which the ſeveral bodies cryſtallize muſt be determined by their relative quantities and attractions. It is perfectly obvious, that no molecules can form a cryſtal in a heat ſufficient for its fuſion; but it by no means enſues, that it will be formed as ſoon as the molecules are cooled to the point where the cryſtalline polarity overcomes the diſintegrating power of heat; for they may remain ſuſpended in a fluid formed by more fuſible bodies, provided this fluid be ſufficiently abundant to keep them from contact with each other, for the cryſtalline polarity appears to exert itſelf only at extremely ſmall diſtances. In a maſs compoſed of ſubſtances in a ſtate of fluidity, with refractory molecules ſuſpended among them, it is pretty clear, from the preceding paragraph, that the moſt abundant ingredient will be the firſt to cryſtallize. But the removal of a portion of the ſuſpending fluid muſt bring the refractory molecules nearer together, and perhaps ſo near that the cryſtalline polarity may overcome the attraction of the fluid for them; they will therefore cryſtallize next, and will be followed by the remaining ingredients, in the order their attractions dictate.

The moſt infuſible cryſtals would not neceſſarily be formed firſt.

As

When refractory ſubſtances cryſtallize after more fuſible matters, they may have the impreſſion of theſe laſt.

As the cryſtals laſt formed muſt neceſſarily be impreſſed, at the parts in contact, by the peculiar forms of thoſe which have been firſt generated, it alſo follows, if the preceding reaſoning be juſt, that the infuſible cryſtals may be found impreſſed by the more fuſible ſubſtance, which cryſtallized firſt; and the remaining ingredients of the mixture, which were ſubſequently arranged, may be moulded on the refractory cryſtals; and thus, in the ſame ſpecimen, may exiſt a refractory ſubſtance generated by fire, impreſſed by more fuſible bodies, and impreſſing them in its turn. From the ſame conſideration it is obvious, that no cryſtal can be formed at a temperature above the degree of its fuſibility; and that, as a neceſſary conſequence, no cryſtal which is more fuſible than the baſis in which it is imbedded, can be formed by igneous operation.

No cryſtal more fuſible than its enveloping maſs, can be formed by igneous operation.

Aqueous ſolution and ſuſpenſion do not differ from thoſe in the dry way. They are effected by the fuſion of the ſolid water, or ice, &c.

The ſame laws muſt regulate the arrangement of aqueous ſolutions, and of molecules ſuſpended in aqueous ſolutions. All theſe are dependant on heat; for we are unacquainted with any fluidity, and conſequently with any ſolution, which heat does not produce. Ice and ſoda have no more action on each other than ſoda and quartz: raiſe the temperature of the ice, and it unites with the ſoda; raiſe the temperature of the ſoda, and it unites with the quartz. Both ſolutions are effected by heat, of the degrees of which we know neither the beginning nor the end, and are therefore utterly unable to eſtimate what aliquot part of its ſcale is adequate to the production of theſe effects. Probably a very minute one.

(To be continued.)

XIV.

Account of the Method of bleaching Cotton, as practiſed at Salzburgh; and the Art of giving a permanent Red to Cotton and Linen. By M. C. SCHOERBING.*

Salzburg method of bleaching cotton thread, &c.

COTTON thread is always waſhed before it goes to the weaver. The method of waſhing here to be deſcribed gives it a much whiter colour than ordinary; and it is equally applicable

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* Journal of Van Mons, No. 15.

Salzburgh method of bleaching cotton thread, &c.

cable to cottons in the piece. This method is called, for what reaſon I know not, the Salzburgh method, though it is chiefly practiſed at Reginſburgh, where four families have long kept it a ſecret, which has not till the preſent occaſion been divulged. This waſhing renders the cotton more flexible; the goods made from it are of a cloſer and more even texture; the cotton does not require any ſubſequent bleaching, and when it comes afterwards to be dyed, it takes a finer, more permanent and more uniform colour. The operation is performed with ſoap, water and ſtrong leys, in the following manner:

The articles to be waſhed, which uſually conſiſt of thread, ſtockings, night caps, and handkerchiefs, are evenly diſpoſed in a large veſſel or tub. The bottom of this veſſel is firſt covered with a coarſe linen cloth, upon which are laid firſt the handkerchiefs, afterwards the ſtockings and night-caps, and laſtly the thread. The whole is then covered with a ſecond coarſe cloth of a cloſe texture, and very clean, which defends the maſs from the contact of the air. The veſſel itſelf reſembles thoſe uſed for lixiviation in the ſoap work of Konſholm. A boiler having its diameter at the top 1½ Swediſh ell (forty inches) and at the bottom 1¼ ell (34 inches) and its depth one ell (27 inches) is filled with water to which a ſufficient quantity of cauſtic leys of potaſh is added to make the fluid produce a greaſy feel between the fingers, and afterwards 2lbs. of ſliced ſoap of Rigenſburg. This liquid is made to boil and poured upon the cotton; a ſhort time after which it is drawn off, to be again heated, during which interval a hot ſolution of ſoap is continually poured upon the cotton. Theſe operations are repeated for four or ſix hours, or until the cotton is well ſoaked, and very hot. It is then left to ſteep for twenty-four hours in ſoap-water. This proceſs is uſually performed between midnight and ſix in the morning; and the next morning the fluid is drawn off, the cotton well waſhed and the water preſſed out by a ſcrew-preſs. This conſtitutes the firſt waſhing.

After the cotton has been well cleanſed in this manner, the principal waſhing is effected as follows:

The waſhed cotton is diſpoſed in the veſſel as before, with this difference only, that the maſs is enveloped in cotton cloth inſtead of linen, which is leſs capable of reſiſting the action of the cauſtic leys. A clear and colourleſs ley is prepared with

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Salzburg method of bleaching cotton thread, &c.

two thirds wood aſhes and one third lime, and concentrated until an egg deſcends ſlowly in it (ſpecific gravity about 1.081). This ley in a boiling ſtate is poured upon the cotton; after which it is drawn off, again heated, and again poured on; and this repetition is continued from midnight to noon, or for twelve hours. The whole of the ley is then drawn off, and the cotton taken out of the veſſel while hot, with the hands defended by gloves, and after ſpreading it upon a table, it is again put into the veſſel, but in a reverſed order. The ſame waſhing is repeated with other leys and continued for twenty-four hours.

Laſtly, the boiler is filled with water, to which 2lbs. of ſoap are added, and the liquid brought to the boiling heat. The cotton is waſhed with this water, conſtantly kept boiling for twenty hours, and is afterwards left to ſteep for ten or eleven hours. The fluid is then drawn off, the cotton taken out of the veſſel, and ſpread on a long wooden table, where it is waſhed and beetled, and afterwards waſhed in a running water. Laſtly, the water is preſſed out by means of a preſs, and the cotton dried upon ſtaves in the ſun, or in damp weather in a room appropriated to that purpoſe.

The remaining leys after theſe waſhings cannot be uſed a ſecond time, but have a blood-red colour; they are mixed together, and are uſed in the common operations of the laundry. The ſoap waters are thrown away.

The ſtaves on which the cotton is ſuſpended muſt be waſhed every time. It is preferable, when the weather permits, to ſuſpend each piece by threads and ſtretched cords in the open air. The pieces ought not to be brought too near together, for fear leſt the places to which the air has not free acceſs ſhould be ſpotted with brown ſpots.

Piece goods and raw linen thread will receive a partial bleaching by this method. It is neceſſary however to make the leys of only half the ſtrength, and the lixiviation muſt be continued only half the time, for fear of weakening the texture.

Linen and cotton as vegetable ſubſtances have the defect of not taking fixed colours. The cauſe reſides in their reſinous principle, of which they muſt be deprived. Alcohol would be a ſure and eaſy ſolvent for this principle, but it is too expenſive. After alcohol, the beſt ſolvents of this principle are oils of every deſcription, but particularly fiſh oil. The cotton is ſoaked in this oil for 24 hours, after having again boiled it for two hours; it is then run and ſuſpended in the air for ſome days, in order that the oil may be well ſeparated, and it is entirely deprived of the oil by a ſtrong hot ley with ſubſequent waſhing and drying. For this effect a ley of two thirds aſhes and one third lime may be uſed, with the ſame quantity of ſheeps dung as of both the before-mentioned ingredients. The cotton is not only boiled in this mixture, but it is alſo applied ſeveral ſucceſſive times; the goods being waſhed in clear water between each lixiviation. The cotton thread may alſo be waſhed in water containing a ſufficient quantity of potaſh to render it greaſy to the feel. The thread is to be boiled for twelve hours in this ley, or till the fluid becomes black. Laſtly, the thread is waſhed and ſteeped in water acidulated with weakly ſulphuric acid, it is to be taken out of this fluid after remaining in it at leaſt an hour, and then waſhed and dried.

If this method of bleaching were once generally known, it would not be neceſſary to purchaſe the article of the bleacher, or to ſend cotton to them to be bleached. In order to aſcertain whether cotton bleached in any manner whatever is perfectly deprived of its reſin, and proper to receive the dye, the glaſs is to be filled with water, and a thread of the cotton placed on the edge of the glaſs, ſo that half its length ſhall be within and half without. If the former end ſinks in the water and the whole thread acts as a ſyphon, by cauſing it to run over, it will be a proof that the cotton will have the requiſite purity.

Red dye for cottons.

The cotton thus purified muſt paſs through three mordants: 1. A decoction of nut-galls. 2. A ſolution of tin; and 3. alum water.

For the decoction of nut-gall. For 1lb. of cotton or linen thread. For the former half a pound of the dark coloured nut-gall, or for the latter 1lb. is taken. The nut-gall is groſsly pounded and boiled with a handful of birch leaves in a copper boiler, with two and half pots of rain water till reduced to one half. The liquid being left to ſettle and poured while yet hot through a cloth on the cotton, this is ſuffered to ſteep for 24 hours, frequently working or preſſing, in order that it may be univerſally penetrated with the galling principle. It is then

Red dye for cottons.

taken out, rung, and dried by ſuſpending it in the open air, or in a heated chamber. It is needful only to take care that no water ſhall drop upon the cotton, as that would produce ſpots in the dye. In this operation the thread muſt be cloſely packed together, in order that the nut-gall may uniformly penetrate its maſs. It is then conveyed into a ſolution of tin, made as hereafter deſcribed, and the decoction of nut-gall muſt be kept to be uſed in the aluming. The ſolution of tin is prepared in the following manner: Sal ammoniac or ſea ſalt in fine powder is to be diſſolved to ſaturation in 1lb. of aqua-fortis; 2 oz. of fine Engliſh tin raſped, are added to this, or as much of the metal as the acid can diſſolve to ſaturation. In another veſſel, 2 oz. of ſea-ſalt are to be diſſolved in one pint of rain water, and the ſolution of tin is to be poured drop by drop with continual agitation into this ſalt water. The galled thread is afterwards put into a ſtone ware veſſel, cloſely preſſed together, and the laſt mentioned mixture poured thereon. The thread muſt be occaſionally compreſſed with the hand, in order to aſſiſt the penetration, and it muſt then be covered from the air and left to ſteep for twenty-four hours, at the end of which time it is taken out, rung and dried in the air or in a heated room, where it may remain for 48 hours. After this it is waſhed in pure water again, dried, and emerſed in the alum-water, of which we ſhall proceed to deſcribe the compoſition.

The remaining ſolution of tin is reſerved for a ſubſequent operation, for which, in that caſe, no more than three quarters of a pound of aqua-fortis and the other ingredients in proportion, need be taken for 1lb. of thread.

The alum water is made as follows: Whatever may be the kind of alum, it muſt be previouſly calcined. One pound of the crude alum is required for 1lb. of thread. When it is calcined it is pulverized and diſſolved in one pint or Engliſh quart of water, and rather more than an equal meaſure of the remaining decoction of nut-gall is added. Theſe being well mixed and heated, are to be poured on the thread, which has been treated as before with the ſolution of tin. It is left for fourteen hours in this bath and afterwards preſſed and dyed.

The dying 1lb. of good bruiſed or ground madder previouſly ſoaked for ſome hours in water, is put into a boiler of ſufficient ſize, which is to be filled with water and placed on

Red dye for cottons.

a moderate fire. As ſoon as the contents begin to heat, the thread is put in and continually turned. It is requiſite that the thread ſhould have been previouſly ſteeped in water, and afterwards wrung, in order that it may be very equally penetrated with the colour. The thread is left in the bath till the liquor boils, after which it is taken out, well waſhed, the threads are arranged on the hand, and the water preſſed out. This firſt dyeing gives the thread a pale red colour. The boiler is then emptied and again filled with water, into which another pound of madder previouſly ſoaked in water is put, and this bath is heated. As ſoon as it has become hot, the thread is put in and boiled for one quarter of an hour. Care muſt be taken that the heat be not too ſtrong. The firſt bath ſhould ſcarcely boil, for fear the articles ſhould take a brown colour; but this inconvenience is not to be feared in the ſecond bath, when the thread is already penetrated with a red colour. After the thread is well waſhed for the ſecond time, it ought to have a deep brown colour; if it has not it muſt be again boiled with a quarter of a pound of madder. After the laſt dye, the thread is to be waſhed by hand in hot ſoap water, after having waſhed it in cold water, and it is then to be dried.

XV.

Report on an artificial Production of Camphor, announced by M. KIND. *Read to the Society of Pharmacy by* BOULLAY.*

BEING charged, in conjunction with M. M. *Cluzel* and *Chomet*, to give an account to the Society of an intereſting phenomenon announced by *M. Kind*, in *Trommſdorff's Journal de Pharmacie*, extracted and reported in the 14th number of the *Journal de Phyſique et de Chimie*, publiſhed by *Van Mons*, I now inform them of the reſult of our labours.

M. Kind's experiment of the production of camphor by common oil of turpentine to abſorb muriatic gas.

" For the purpoſe of procuring the arthritic liquor of *Pott*, which is preſcribed here with advantage, for reſolving the nodoſities and other obſtinate tumours which form upon the joints, I intended," ſays *M. Kind*, " to incorporate the muriatic gas with

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* From Annales de Chimie, Fructidor, No. 153. Vol. LI.

with oil of turpentine, at the inſtant of the diſengagement of the gas. This operation ſhewed me a phenomenon which, I am of opinion, is deſerving of being known.

"I put decrepitated muriate of ſoda into a tubulated retort; I adapted two of Woulf's bottles to the retort, into which I had put a quantity of oil of turpentine, equal in weight to the ſalt. On this I poured half the proportion of concentrated ſulphuric acid, and I diſengaged the muriatic gas by a graduated fire. At firſt the oil acquired a yellow colour, afterwards a pale brown colour, which, towards the end turned to a deep brown. After cooling, the liquor was almoſt wholly coagulated into a cryſtalline maſs, which, in every reſpect, comported itſelf like camphor.

"I do not think," ſays M. Kind, "that a ſimilar production of camphor has ever been noticed before; but Meyer ſpeaks of a concretion of a camphoric nature, formed in oil of turpentine digeſted with dry cauſtic ſalt." He afterwards inſtances a formation of camphor obſerved by the late M. Trommſdorff, on diſtilling eſſential oils on lime. "It is probable," ſays M. Kind, "that the great tendency of muriatic gas to combine with water, determined the union of the principles of this liquid contained in the oil, ſo that the carbon became predominant, and made the oil brown; and, that the effect of this acid gas on it, correſponded with that obſerved by Achard, in the action of concentrated ſulphuric acid on fat oils. He did not remark that the heat was as conſiderable during the action of the gas as Woulf ſtated it to be."

Repeated by M. Trommſdorff.

In an additional note, M. Trommſdorff announces that he had repeated M. Kind's experiment, and that he had obtained analogous reſults.

He dried the concrete matter between ſheets of paper; it became very white, ſemi-tranſparent, and poſſeſſed the following properties:

"Its odour was ſtrong, very analogous to that of camphor, but injured by that of the turpentine.

"Placed in a ſilver ſpoon, and heated over red hot coals, it volatilized without leaving any reſidue, and yielding a ſtrong ſmell of camphor.

"Its vapour inflamed.

"Expoſed to a gentle heat, in a ſmall phial, it was entirely ſublimed. It diſſolved with facility in oil of almonds.

"It was entirely diſſolved by alcohol, but not ſo ſlowly as common camphor, and was precipitated unaltered by water.

"Concentrated nitric acid affected its ſolution, at firſt tranquilly, but afterwards with a diſengagement of nitrous gas and heat. The mixture became turbid by the addition of water, but did not depoſit camphor."

He concludes from theſe experiments that this product comported itſelf exactly like camphor, except with the nitric acid; but he believes that this difference may depend on a ſmall quantity of muriatic acid, or a little oil of turpentine, from which it is very difficult to free it entirely.

M. Trommſdorff afterwards diſengaged the gas from a quantity of muriate of ſoda equal to that employed in the firſt experiment, through the matter remaining in the bottle; but he found that inſtead of augmenting, the camphorized part diminiſhed in quantity, loſt its camphoric odour, and the alteration of its other properties induced this chemiſt to believe that the new addition of gas had cauſed it to undergo a decompoſition.

Repetition of theſe experiments by the reports.

The above experiments, repeated with the greateſt attention, gave us ſatisfactory reſults, even with the different ſpecies of oil of turpentine of commerce.

The importance of the ſubject, and the objections made by ſome members, at the reading of the obſervation which lead to this report, made us ſenſible how much the labours of M. Kind left to be wiſhed for; the quantities of the product alſo not being indicated, we thought we ſhould accompliſh the intention of the Society, in following the track marked out by the author, if we endeavoured to add ſome facts to thoſe he has publiſhed, and we propoſed the following queſtions for our guidance.

Queſtions for inveſtigation.

1ſt. Does the gazeous muriatic acid ſerve to determine the re-action of the principles of the oil of turpentine, and to change their proportion, to form the camphor; or, is its action limited to inſulating this immediate principle, from the oil with which it was not combined?

2nd. What is the quantity of the camphoric product?

3rd. Will a greater quantity of the gas than that indicated by M. Kind add to the product, or will it deſtroy it?

4th. Will another quantity of gas added to the liquor which has yielded the camphor, and from which it has been ſeparated, determine a new formation of this principle in it?

5th. What are the beſt means of purifying this camphor, and of freeing it from the odour which it retains?

6th. What are the relations and differences between the purified camphor of turpentine, and that of the camphor-laurel?

7th. Would liquid muriatic acid produce the ſame effect?

8th. Would the other acids act in the ſame manner on this oily ſubſtance?

The complete ſolution of theſe different queſtions would doubtleſs have required a great number of experiments, to which we were unable to attend: we ſhall only relate thoſe which we made.

Apparatus. With a view to obtain a more conſiderable product, which would yield us the concrete matter in a quantity ſufficient to be aſcertained and ſubjected to different experiments, we prepared an apparatus ſimilar to that uſed by the author of the diſcovery, but of larger dimenſions. It conſiſted of a tubulated retort, and two Woulf's bottles, provided with their tubes of communication and ſafety. **Proportion of ingredients.** Four pounds of marine ſalt, and two pounds of concentrated ſulphuric acid were introduced into the retort; four pounds of very white oil of turpentine were poured into the firſt bottle, and two pounds of diſtilled water into the ſecond. Fire was placed under the mixture, and augmented gradually till the entire diſengagement of the muriatic acid gas, which was totally abſorbed by the oil. **All the gas was abſorbed. Changes of the oil.** The latter exhibited the following phenomena: it became at firſt of a citron colour, and afterwards browniſh, which grew gradually darker towards the end of the operation; it retained its tranſparency; it grew very hot; its volume augmented about a tenth; the ſides of the bottle were covered with ſmall limpid drops which united ſlowly at its bottom, in the form of a thick oil.

Cryſtalline product by repoſe. Left to itſelf for twenty-four hours, it formed a maſs of irregular cryſtals, from which ran a brown liquid. When dried and ſtrongly preſſed between ſheets of paper, this cryſtalline matter was very white, and of a peculiar odour, participating of the oil and the acid made uſe of; it weighed twenty-four ounces.

More cryſtals by cold. The liquor which had been ſeparated from theſe cryſtals, expoſed in a cellar for ſeveral days, yielded others, which dried like the firſt, were of equal whiteneſs, and weighed four ounces.

Seven and half oz. of camphor from one pound of oil of turpentine.

To aſcertain whether there did not yet exiſt a further quantity of this concrete matter ready formed, in this ſort of mother-water of camphor, it was expoſed to an artificial cold of eight or ten degrees below 0, during ſeveral hours. By this means it yielded other cryſtals, which, collected like the preceding, offered the ſame characters, and weighed two ounces; this brings the quantity of camphor to ſeven ounces and a half for each pound of oil of turpentine.

More gas did not produce cryſtals in the reſidual liquor.

This liquor, not appearing capable of yielding more cryſtals, was divided into two portions, one of which was reſerved for examination in this ſtate, the other was ſubjected to the action of a new quantity of acid gas; it diſſolved a part of it, and became almoſt black, without loſing its tranſparence, even after remaining for ſeveral days in the cellar.

Double quantity of gas.

Another doſe of oil of turpentine was charged with a proportion of gas, double that mentioned above. The precipitation of camphor took place in nearly a ſimilar quantity; the liquor was very little more coloured, but a much greater quantity of fat oil was formed.

Action of diſtilled water on this camphor;

An ounce of this matter waſhed in diſtilled water, and put in a filter to drain, became of the moſt beautiful white; it no longer gave ſigns of acidity, but had ſtill an odour of turpentine. The water in which it had been waſhed was very aromatic; it reddened the tincture of turnſole, and precipitated by nitrate of ſilver.

and of water acidulated with carbonate of potaſh.

An equal quantity of the ſame ſubſtance was waſhed with water impregnated with a fiftieth part of unſaturated carbonate of potaſh: it loſt much of its firſt odour.

Action of charcoal, aſhes, quick-lime, and alumine.

New portions of this rough camphor were mixed in equal parts, one with charcoal in powder, another with very dry aſhes, a third with quick-lime, a fourth with alumine *(porcelain earth)*, and were introduced ſeparately into ſmall glaſs alembics, the capitals of which were, by a gentle heat, rapidly covered with diſtinct groupes, compoſed of a multitude of ſmall needle-formed cryſtals, diverging from the ſame center.

Other properties.

Purified in this manner, it entirely loſt the odour of oil of turpentine; that which it retained was more analogous to common camphor, but not ſo ſtrong. In this ſtate it ſwims on water, to which it communicates its taſte; it burns on its ſurface, without experiencing the revolving motion obſerved by

Profeſſor Venturi. The only motion we obſerved was the reſult of the molecular attraction, which determined a great number of the fragments diſperſed over the ſurface of the water to collect in one point.

Is ſoluble in alcohol.

It is wholly and readily diſſolved in alcohol, from which water ſeparates it unaltered.

Action of dilute nitric acid.

Nitric acid, at thirty degrees of *Beaumé's* areometer, poured on this new camphor, had no action on it, even after ſeveral days of contact; although the ſame acid diſſolves the common camphor with the greateſt facility, becoming covered with an oily ſtratum, and water precipitates pure camphor from it.

Of concentrated nitric acid.

Very concentrated nitric acid had not, at firſt, any action on our camphor; but after a few ſeconds the ſolution is effected, and at the ſame time a diſengagement of nitrous gas is manifeſted: water rendered this ſolution very ſlightly turbid. The ſame acid, on the contrary, diſſolved the camphor of the ſhops rapidly, diffuſing white vapours. This ſolution took a beautiful fire-red colour, and water ſeparated pure camphor from it in flocks.

It is not diſſolved by acetic acid.

Neither did acetic acid, which perfectly diſſolves the common camphor, effect the ſolution of this. By heat, it ſoftened and ſeemed to diſſolve; but on cooling, it was wholly collected on the ſurface of the liquid, with all its properties.

We afterwards examined the liquor called the *mother-water of camphor*.

Phyſical properties of the mother-water. Peculiar oil.

This brown tranſparent liquor, lighter than water, heavier than alcohol, fuming by contact of the air, of a peculiar odour, ſwam above a thick reddiſh oil, the ſmall quantity of which obliged us to neglect the examination of it.* The taſte of this mother-water was ſharp; it did not mix with water, nor yield its acid to it. Agitated in this liquid, its particles remained diſperſed for a long time, before they collected on its ſurface.

Soluble in alcohol and ether.

Alcohol and ether diſſolved it completely; but the addition of water ſeparated them.

Inflammable.

Inflamed in a capſule, it burns without a reſidue, diffuſing a conſiderable quantity of very odorours ſmoke.

Action of ſulphuric acid,

Concentrated ſulphuric acid poured into this liquor, blackened it, and diſengaged from it ſome vapours, and a very ſtrong odour of gazeous muriatic acid.

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* We only aſcertained its ſolubility in alcohol and ſulphuric ether, and its inſolubility in water.

A ſolution

and of carbonate of ſoda.

A ſolution of cryſtallized carbonate of ſoda produced a ſlight efferveſcence in it, diſcoloured it, rendered it lighter, and formed muriate of ſoda.

Products by its diſtillation.

This muriated oil, diſtilled to dryneſs in a ſuitable apparatus, yielded ſome muriatic gas, ſome drops of thick oil, ſimilar to that mentioned above, a very light and very combuſtible perfumed liquid, which comported itſelf with water, alcohol, ſulphuric acid, and ſoda, in the ſame manner as before diſtillation; there was only leſs muriatic acid in it. The reſidue was a black matter, glutinous like pitch, diſſolving imperfectly in alcohol, and yielding muriatic acid to water.

Mutual action of oil of turpentine and muriatic acid produces a ſmall quantity of a concrete body.

We afterwards made a mixture of equal parts of oil of turpentine and muriatic acid at 25°. We agitated it from time to time, and then left it for ſeveral days. The oil took a reddiſh brown colour, the acid alſo became reddiſh: both of them preſerved their tranſparence, and at the point of contact of theſe two liquids, was perceived a concrete ſtratum, having ſeveral of the properties of camphor, but its quantity was ſo ſmall that we were obliged to neglect it.

Previous diſcovery of this fact.

A fact of this kind is to be met with in the tranſlation of the Chemical Recreations of *Model*, publiſhed in 1774, by M. *Parmentier*.

This philoſopher expreſſes himſelf as follows, in page 400 of the firſt volume of this tranſlation, in the additions which he has made to it:

“A very curious and very intereſting obſervation to natural philoſophy and chemiſtry, is, that M. *Marges*, ſurgeon, in a work, the object of which is the examination and chemical analyſis of different medicaments *, ſays, that he obtained, from the digeſtion of a mixture of fuming marine acid and oil of turpentine, ſmall ſaline concretions, which at the end of a certain time, became more conſiderable, and took the figure of a cryſtalline ſalt, in form of a parallelopipedon, while the oil which ſwam above them was coloured red, and acquired a very thick conſiſtence, &c.

It is very probable that theſe cryſtals were the ſame as thoſe which were obtained in ſuch great abundance from the action of the ſame gazeous acid.

Oxigenated muriatic acid gas and oil of turpentine.

The ſame oil was ſaturated with oxigenated muriatic acid gas; it acquired a deep yellow, retained its tranſparence, became a little more conſiſtent, but did not produce any camphor.

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* Paris, 1774. Second edition.

No camphor.

Action of ſulphuric acid.

Concentrated ſulphuric acid acting too violently on oil of turpentine, the decompoſition of which it ſpeedily effects, we made a mixture of equal weights of this oil and ſulphuric acid marking only 40° of the areometer. The action was confined to the point of contact, and was analogous to that of liquid muriatic acid.

Slow diſtillation of oil of turpentine.

Eight ounces of oil of turpentine were kept for three days in a glaſs alembic, placed on a ſand-bath, the temperature of which was maintained at about forty degrees of *Reaumur's* ſcale. This time being elapſed, we found in the recipient nearly four ounces of a very white and very light volatile oil, and in the capital many ſmall cryſtallizations of true camphor. The oil remaining in the cucurbit was of an amber colour, and very much thickened.

Action of muriatic acid gas on the other volatile oils.

To ſatisfy ourſelves whether the other volatile oils would furniſh camphor by the action of the muriatic gas, we incorporated them with it in different proportions; but did not obtain any precipitation. They became very black, and contained a great quantity of heavy oil, &c.

CONCLUSION.

General reſults.

1ſt. That the production of camphor, announced by M. *Kind*, is uniform in the proportions he indicates, and of whatever ſoil the oil of turpentine may be.

2nd. That the camphoric product may be eſtimated at about half the quantity of the oil employed.

3rd. That a greater quantity of the gas does not add to the product, nor does it deſtroy it.

4th. That this camphor may be purified by water, by alkaline ſolutions, charcoal aſhes, quick-lime and alumine: and, that theſe three laſt ſubſtances are more eſpecially preferable.

5th. That it differs from what is extracted from the camphor-laurel, in its taſte, which is not ſo bitter, and in its odour, which is leſs penetrating; and, that it preſents an abſolute difference in its mode of action with the nitric and acetic acids, the firſt of which only diſſolves it by a reciprocal decompoſition.

6th. That, notwithſtanding theſe differences, it is not the leſs a beautiful new ſpecies of this immediate principle of vegetables to chemiſts; and, that it is to be wiſhed that phyſicians would try it, to aſcertain whether, as there is reaſon to believe, its action will be the ſame on the animal economy.

7th. That, during the operation from which this camphor reſults, there is formed a true combination of muriatic acid and volatile oil, having a reſemblance to what the ancients called *acid ſoaps*.

8th. That liquid muriatic acid, and dilute ſulphuric acid, not mixing with oil of turpentine, their action is confined to their point of contact.

9th. That the mode of action of the muriatic gas, in this circumſtance, may be explained by the elegant theory which MM. Fourcroy and Vauquelin have given of the action of ſulphuric acid on vegetable ſubſtances in general.

10th. That, notwithſtanding camphor is ſet free by the ſlow diſtillation of oil of turpentine, without addition, we are led to believe that ſo large a quantity of this matter could not exiſt ready formed in it, and that at leaſt a part is the reſult of the re-action of the principles of the oil, excited by the preſence of the acid gas, and its affinity for water.

11th. Finally, that the ſame proceſs is not applicable to the extraction of the camphor contained in the volatile oils of lavender and roſemary.

XVI.

Letter from JOSEPH HUDDART, *Eſq. F. R. S. on the apparent Enlargement of the Moon at low Altitudes.*

To Mr. NICHOLSON.

DEAR SIR,

IT was not until yeſterday that I received your Journals, Nos. 36 and 37: In the former, reading a letter from C. L. occaſioned my referring to No. 35, page 164, of the Journal, for Mr. Walker's letter reſpecting the ſenſible magnitude of the Introduction.

the horizontal moon above what it has in the zenith; and therefore preſent you with the idea I have entertained upon the ſubject.

Eſtimate of magnitude confined to the ſize of the image on the retina.

I am of opinion that the magnitude of any object can only be eſtimated (as to ſenſe) according to the ſpace or magnitude of its image upon the retina. The eye muſt (if this is admitted), therefore, be the ſole cauſe of the illuſion, for the moon's apparent magnitude increaſes as ſhe riſes towards the zenith.

The eye is formed not only to alter its aperture, but alſo its focal diſtance.

Elongation of the eye will enlarge the image, though the angular magnitude remains unaltered.

The eye by the power of the muſcles can, at the will of the obſerver, not only be directed towards the object, in order to receive its image upon the uſual part of the retina, but acquire a proper convexity for diſtinct viſion, or focus of the refracted rays upon the retina; and alſo to contract or extend the area of the pupil according to the quantity of light. This admirable property may be obſerved by every one in paſſing out of a light room in the night, and alſo in the eyes of the brute creation; but I conceive, that while the area of the pupil is enlarged in order to receive a greater quantity of light, the eye is protruded, or the focal diſtance between the pupil and retina is increaſed, which increaſes the ſenſible magnitude of the object, as the image upon the retina conveys it to our ſenſes, when viewed by the naked eye. This I diſtinguiſh from apparent magnitude, which we uſe when actually meaſured by an inſtrument.

Variation of the pupil, or of the aperture of a lens, cannot alter the image.

The variation in the magnitude of the image upon the retina, certainly cannot ariſe from the variations in the dimenſions of the pupil, which is contrary to the law of dioptrics; for, on obſerving the ſun, I do not admit light through one-twentieth part of the area of the object glaſs, which is 3.8 inches in diameter, and yet there is not any difference in the apparent diameter; or, whether the rays are admitted at the center or any other part of the object-glaſs, the obſervations are equally good. But I conſider it as a natural conſequence, that when the pupil of the eye is enlarged, the focal diſtance is increaſed, and which conſtantly ariſes from a dimiṇution of light, even without our attention; but thereby our ſight is aſſiſted in conſequence with more light and power, which is ſome compenſation.

The elongation of the eye is ſuppoſed to take place whenever the pupil is enlarged;

and as this takes place when the moon is leaſt lu-

The light from a celeſtial object near the horizon, is diminiſhed by paſſing a long diſtance through a groſs atmoſphere;

hence a ſenſible enlargement takes place in the moon as well as other bodies, which diminiſhes as the altitude increaſes, and the light leſs interrupted.

minous, it then appears largeſt.

I am, Sir,
Your moſt obedient,
J. HUDDART.

Highbury Terrace, Jan. 28, 1805.

XVII.

A Memoir on Milk and the Lactic Acid. By Cit. Bouillon Lagrange *.

SECTION I.

The preſent State of our Knowledge reſpecting the Lactic Acid.

SCHEELE and Deyeux, and Parmentier, are the chemiſts who have moſt contributed to our knowledge of the acid of milk.

History of experiments on milk, and the authors of diſcoveries, &c.

The experiments made before the time of theſe ſkilful philoſophers, are thoſe of Hoffman, Boerhaave, Homberg, Geoffroy, Rouelle the younger, and Baumé. But the analyſis of animal matters was then in ſuch an imperfect ſtate, that little can be gathered from their experiments.

Scheele directed his labours with very different views. His wellconducted experiments led him to determine the characters and properties of an acid afforded by the ſerum of milk But notwithſtanding the labours of this celebrated man, we have ſome ſubjects of inquiry left for examination.

Scheele.

Beſides the great number of its ſaline compounds, as Fourcroy remarks, which are ſtill wanting to the ſcience, Lehale has

Imperfect ſtate of our knowledge of the lactic acid.

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* Annales de Chemie, No. 150. vol. 50.

The author in a note mentions his having learned on the evening when his Memoir was read, that Meſſrs. Vauquelin and Thenard had each been ſeparately employed on the ſame ſubject: and on that occaſion he then finds its neceſſary to ſay, that moſt of the experiments he deſcribes, are the reſult of obſervations made by ſeveral pharmacians, at the ſitting of the ſociety of pharmacy, the 15 Nivoſe, in the year 12. The verbal proceſs of that day, ſigned by Parmentier, preſident, and Delunel, ſecretary, of which an extract was ſent to the Philomathic Society, aſcertains the experiments he had then made, and the obſervations of of the ſeveral members of the ſociety of Pharmacy.

not pointed out the action of fire upon this acid, nor its ſpontaneous alteration in the air, the manner in which it comports itſelf with the nitric acid, &c. It is not known whether it be totally decompoſed by this laſt, or converted into ſome other acid, particularly the oxalic acid. We are entirely unacquainted with the nature of its compoſition. Though it preſents properties which reſemble thoſe of the acetous acid, and lead to the probability that it nearly reſembles it; we cannot yet rank it with the vegetable acids. On the other hand, nothing deciſive can be aſſerted with reſpect to its animal nature, becauſe no experiment has yet indicated the preſence of azote, and it is yet unknown, whether it may afford ammonia in its decompoſition; whether it be putreſcible, or convertible into pruſſic acid, &c.

This general ſtatement while it gives us a view of our knowledge of the lactic acid traces, as it were, the ſteps neceſſary to be purſued in determining the place which this acid ought to occupy, as to the number of well eſtabliſhed chemical facts.

SECTION II.

Concerning Milk and the caſeous Matter.

On milk, and its conſtituent parts.

Before I proceed to deſcribe the experiments I have made on this article, it will be uſeful to preſent ſome reflections on milk, and its conſtituent parts.

Guiton, in the Encyclopédie Methodique, offers two queſtions reſpecting the exiſtence of an acid in milk. He expreſſes himſelf thus:

Whether whey exiſts ready formed in milk.

Does the whey exiſt in milk ſuch as it is found after the ſeparation of the other conſtituent parts? Does it manifeſt acid properties on any other account, than becauſe it holds ſalts in ſolution, as all the analyſes ſuppoſe?

Milk cannot be reproduced by the mixture of its parts.

If whey, adds the ſame chemiſt, exiſted in milk, in the ſtate it exhibits after the ſeparation of the butter and cheeſe, we ſhould be able to reproduce milk by mixing theſe three principles again in the ſame proportions. But as this is not the caſe, he concludes, that whey is the product of a true fermentation.

The examination of this firſt queſtion founded on experiment, ought, I think, to prove that it will not be ſufficient to ſhew whether whey exiſts in milk, that we ſhould mix the three principles in the ſame proportions, and re-produce that fluid; firſt, becauſe theſe principles are no longer the ſame;

and ſecondly, becauſe whey formed by the uſual proceſs, is more or leſs acid.

Experiment 1. Recent milk gives a red colour to the paper and the tincture of turnſole.

Exp. 1. Milk reddens turnſole.

Experiment 2. If milk be diſtilled in cloſe veſſels, and the product ſeparately taken, the firſt is not acid; the ſecond reddens turnſole, and ſlightly precipitates the nitrate of ſilver; and the third has no action on the tincture. At this period, the milk is not yet decompoſed, but ſtill reddens turnſole. It appears that the remaining acid is no longer volatile, but is retained either by the animal matter, or by ſome other ſubſtance.

Exp. 2. It affords acid by diſtillation, and continues acid.

Experiment 3. If milk be coagulated with a mineral or vegetable acid, the whey is ſeparated, without retaining a particle of the acid made uſe of.

Exp. 3. Whey of milk coagulated by an acid, retains none of the acid itſelf.

I muſt however obſerve, that ſome deception would follow, if the ſerum only were examined, which is obtained by the ſulphuric acid, or by allum, becauſe the precipitate afforded by barytes, is not entirely re-diſſolved by the nitric acid. I am ſatisfied that this ariſes only from a ſmall quantity of ſulphurate of potaſh contained in the ſerum, as is evidently proved, when the whey is ſeparated by any other ſubſtance not of an acid nature.

It is not true therefore as many chemiſts have advanced, that the acid ſeparates the caſeous matter, by uniting with the ſerum.

Experiment 4. If milk be expoſed to the air at the temperature between 12 and 20 degrees (about 66° Fahrenheit) the ſeparation takes place in twenty-four hours. The curd has a more acid flavour than that of experiment 3. Hot water cauſes it to loſe its ſour taſte, and acquires the property of reddening turnſole.

Exp. 4. Whey from milk ſpontaneouſly decompoſed, is alſo acid.

We here find nearly the ſame properties in the cheeſy matter, when ſpontaneouſly formed, and when ſeparated by acids.

Experiment 5. The ſame experiment being made with the pneumatic apparatus, was attended with no abſorption of air, or diſengagement of elaſtic fluid. The only difference was, that the ſeparation of the cheeſy matter did not take place for ſeveral days. Part floated on the liquid, and the other occupied the bottom of the veſſel.

Exp. 5. Milk ſpontaneouſly decompoſed, neither abſorbs nor gives out air.

Experiment 6. A bottle was filled with freſh milk, and well corked. Some days afterwards, the cheeſy matter had ſeperated. As ſoon as the ſeparation was complete, the cork was intended to be extracted, but it flew out with violence and

Exp. 6. Milk decompoſed ſpont. in a cloſed bottle, gives carbonic acid.

noiſe, indicating the preſence of an elaſtic fluid. Upon ſhaking the bottle, a greater quantity of gas was diſengaged, which being collected and examined, had the ſame characters as carbonic acid.

The fluid as well as the curd had a ſharp acid taſte, which became leſs pungent, as the carbonic acid was diſengaged by agitation.

The fluid, after having been heated, did not appear to be more acid than ſerum, obtained by expoſing milk for the ſame time to the air.

The ſame experiment was made with milk which had been boiled about half an hour, and the reſults were the ſame.

Exp. 7. Qualities of the cheeſy matters.

Experiment 7. The cheeſy matters of the third, fourth, and fifth experiments, differ eſſentially in taſte and conſiſtence.

That of No. 3. is dry and firm; that of No. 4. is more divided, leſs dry, and partly ſoluble in water; and laſtly, that obtained in the experiments No. 5 and 6. is not clotted, but is more light, and does not unite into a maſs, till after ſeveral hours.

The cheeſy matter, therefore, requires particular properties, according to the ſubſtances and the proceſſes employed to ſeparate the ſerum.

Deductions from the facts.

Theſe firſt experiments lead us to the following reflections, that, 1. Milk does not require to be decompoſed, in order to manifeſt the preſence of an acid. 2. That this acid is mixed with ſalts, ſugar, and animal matter. 3. That the acid in milk is diſengaged, though it is not very perceptible, but by re-agents. 4. That the contact of the air is not neceſſary for ſeparating the conſtituent parts of milk. 5. That the coagulating ſubſtances merely facilitate the ſeparation of the cheeſe, either by forming a new compound, or by more immediately condenſing the particles together, when the cheeſy matter requires new properties: or laſtly, by expoſing the milk to the air. By degrees, a part of the ſugar of milk is decompoſed; carbonic acid is formed, of which one part is diſengaged, and the other facilitates the ſeparation of the curd; caloric alſo favours the attractions of coagulating matters for the curd. They all act differently, for alcohol, which alſo poſſeſſes this property, affords other reſults; the curd being to a certain point ſoluble in water. We cannot therefore as chemiſts have aſcribed theſe effects to the ſolution of thoſe ſubſtances in water, and their greater attraction for the liquid than is exerted by the cheeſy matter itſelf.

(*To be continued.*)

Outlines of Pendulous measures of time
By J. Whittley Boswell Esq.

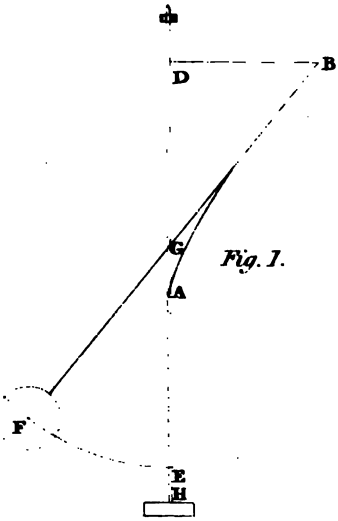


Fig. 1.

Fig. 4.

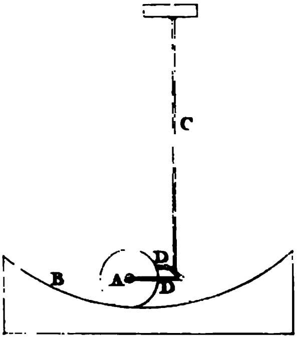


Fig. 2.

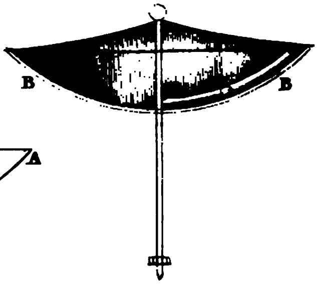
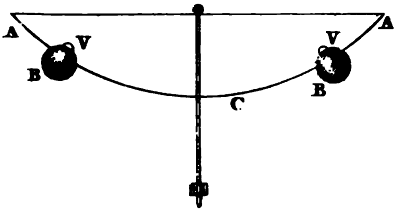


Fig. 3.



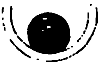


Fig. 5.

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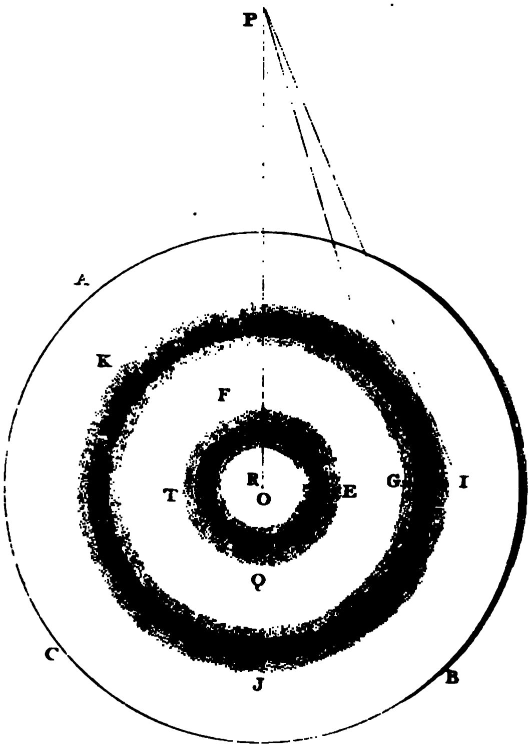
Mr. Gough's theory of the Augmentation of Sound by communicated vibrations.



Medow Sc. Bg. Coll. Gt.

Capt. Mortlock's Method of surveying, by very simple Instruments.

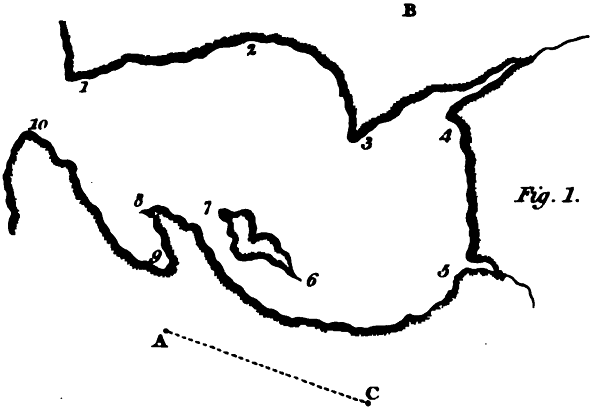


Fig. 1.

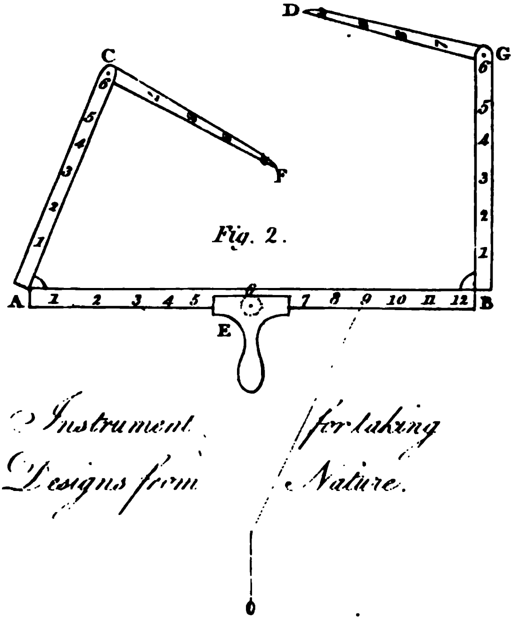


Fig. 2.

Instrument for taking Designs from Nature.

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A
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THE ARTS.

MARCH, 1805.

ARTICLE I.

Hiſtorical and Critical Obſervations relating chiefly to the Invention of the Teleſcope. In a Letter from E. O.

To Mr. NICHOLSON.

SIR,

Incompreſſibility of water.

ALTHOUGH I was aware that the experiment on the incompreſſibility of water was to be found in Lord Bacon's work, yet I was happy to ſee that Sir H. Englefield had taken the trouble of reminding your readers of the original inventor of it. I am always glad when I ſee any thing which may recall our attention to the works of our illuſtrious countryman. They abound with inſtruction, and, in many inſtances, his conjectures may be compared to the Queries of Sir I. Newton, which contain the foundations of the greateſt diſcoveries.

Remarks on the communications of Sir H. C. Englefield and Aletes.

In the ſame paper Sir H. Englefield has given us a very curious paſſage from Kepler; and although I cannot perſuade myſelf that he is accurate in his interpretation of it; yet I think the argument has been by no means accurately ſtated by Aletes *. I ſhall therefore beg your permiſſion to inſert ſome additional remarks on a ſubject, which certainly poſſeſſes a conſiderable ſhare of intereſt. I muſt beg leave, likewiſe,

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* Vol. X. p. 92.

Obſervations relating to the invention of the teleſcope.

previouſly to remark that Sir H. Englefield has not aſſerted that Kepler was the original inventor of the teleſcope. The only inference which can be juſtly drawn from his words, is that they were invented as early as the year 1607. No one, indeed, who had attentively read Kepler's dedication of his Dioptrice, could have entertained ſuch an opinion. He preciſely ſtates * that ſince " ad magnum cumulum inventionum hujus ultimi ſæculi acceſſiſſet Arundo Dioptrica, nequaquam inter vulgares commemoranda machinationes, circaque eam, alii de palma primæ inventionis certarent, alii de perfectione inſtrumenti ſeſe jactarent, ... Galilæus vero ſuper uſu patefacto in perquirendis arcanis Aſtronomicis ſpeciociſſimum triumphum ageret; . . . . Ego ductus honeſta quadam æmulatione novum mathematicis campum aperui . . . . . cauſarum lege geometrica demonſtrandarum quibus . . . effectus inniterentur."

Before the invention of teleſcopes, aſtronomical obſervations were ſometimes made with tubes, but more generally with rods (regulæ) which had ſights fixed at each end. Theſe inſtruments were generally called Dioptra, and the apertures in the ſights were called foramina dioptrorum, not perſpicilla. Ptolemy in his Almegiſt † deſcribes the inſtrument, which is generally known by the name of his Triquetrum. It had ſights at each end, with apertures in them, which Georgius Trapezuntius, the tranſlator, calls foramina: he mentions, indeed, no particular name for this part of the inſtrument, but Theon in his commentary ‡ deſcribes the conſtruction of it to be ſuch, that the whole moon would appear through the ſights εν τη διόπτρα. Proclus Diadochus, in his Hypotypoſis Aſtromicarum Poſitionum,** mentions the Dioptra of Hipparchus, which was an inſtrument of the ſame kind: ſee Riccioli Almegiſtum Novum, vol. I. B. 3. C. 10. § 4. But we can have no further doubt upon the ſubject, if we turn to Flamſtead's Hiſtoria Cæleſtis, vol. III. p. 97. He there ſays that " haud pauci obſervationum

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* Pp. 53, 54. Lond. 1653.   † Book 5, chap. 12.

‡ P. 258. Baſil, 1538. Notwithſtanding what Harduinus ſays, I have no doubt, but that the Dioptra mentioned by Pliny in his Natural Hiſtory, B. 2 C. 69. was an inſtrument of this kind; but no deſcription of it is given, and therefore I cannot argue upon it in this place.

** P. 399. Baſil, 1541.

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obſervationum Tychonicarum errores ex nudorum Dioptrorum uſu neceſſarie conſequuntur." Now it is only neceſſary to examine the Aſtronomiæ Inſtauratæ Mechanica, and we ſhall ſee that almoſt all the inſtruments, which Tycho Brahe uſed, are fitted with ſights, which were ſome perforated with ſmall holes, and ſome divided by narrow ſlits: this part of the inſtrument he called *dioptra*, the perforations he called *foramina*, and the ſlits *rimulæ*.

I could quote further proof that theſe words were commonly uſed in the ſenſes which I have affixed to them, but it would be not only unneceſſary but tedious. It muſt, therefore, remain with Aletes to produce as good authorities for the alledged difference between *perſpicillum* and *perſpicillum vitreum.* I cannot help thinking, however, that the very title-page of Kepler's Dioptrice will be fatal to his explanation; for there we find that mention is made of the diſcoveries of Galilæo, made "*ope perſpicilli*" after the publication of the Siderius Nuntius. Here the word *perſpicillum* is undoubtedly uſed for the teleſcope itſelf, and before I conclude what I have to ſay, I ſhall quote a paſſage from Galilæo, which is ſtill more contradictory to the ideas of Aletes.

Although I differ from your correſpondent with reſpect to the arguments, which he has ſtated to you, ſtill I agree with him in his general concluſion, that Kepler did not mean a teleſcope by the word *perſpicilla.* We learn, indeed, from Borelli's book de vero inventore teleſcopii, that * one account makes the invention as early as the year 1590; but when we compare this with the depoſition of Sara Gædarda † and what is ſtated by Galilæo in (p. 10 of) his Sidereus Nuntius, it ſeems moſt probable that teleſcopes were not known before the year 1609. If Zachary Jauſens was acquainted with them before that period, he ſeems not to have publiſhed his diſcovery to the world. Now Kepler, in the place above quoted from the dedication of his Dioptrice, takes an opportunity of praiſing the invention in the higheſt terms, and as he did this, when it was public, it is probable that he would have done ſo ſtill more warmly, if he had been the firſt who publiſhed an account of obſervations made with them; but there is nothing of this kind in the paſſage alluded to in his book de cometis.

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* P. 25. † p. 31. This makes the invention about 1611 or 1613, but "de certo præfixo tempore non potuit dicere."

Obſervations relating to the invention of the teleſcope.

He was looking at the ſtars with a friend on the bridge at Prague, and they obſerved one in particular. "Vidi"* he ſays, "ſtellam ſub urſa, majorem cæteris per perſpicilla intuitus, quæ æquale cæteris fixis lumen mihi ſine perſpicillis diffundere videbatur." It is not likely that the perſon, who mentioned the firſt uſe of ſuch an inſtrument as a teleſcope, would confine himſelf to ſo dry an account of the effects produced by it. But there is another argument, which may be drawn from the book itſelf, and that is, when Kepler is giving an account † of his obſerving a comet in the year 1618, he uſes the word teleſcopium and not perſpicillum. "6 Sept. jam nulla amplius cunda viſu nudo potuit obſervari et teleſcopio inſpectus ſatis magnus apparuit." "Sept. 23. Planitudo major ſine teleſcopio, quam per illud." ‡

But what puts it beyond doubt that Kepler was not in poſſeſſion of a teleſcope ſo early as the year 1607, is that we hear of no great diſcoveries which he made about that time by obſerving the heavens. Monſieur de Moutucla, indeed, ſays § that "Uniquement appliqué a determiner avec preciſion les mouvemens celeſtes, cet homme celebre faiſoit peu d'uſage du teleſcope." He probably thought himſelf juſtified in this, when others were employed in the more mechanical labour of obſervation. When he had no ſuch fellow-labourers, it is impoſſible that a man like Kepler could have neglected an opportunity of obſerving, or have concealed the means of doing ſo; and if he had not done both, we muſt have had ſome notices remaining either in his own writings or thoſe of others.——We muſt recollect likewiſe, that he was the firſt who underſtood the theory of teleſcopes and the combination of lenſes, and we muſt not forget, that theſe diſcoveries being once made, the greateſt difficulty was overcome; it was only neceſſary to uſe the teleſcope and mark the objects which it preſented to the ſight.

From what I have ſtated there ſeems to be conſiderable difficulties attending either of the ways, which have been propoſed for interpreting the word perſpicilla. But there are ſome paſſages in the Sidereus Nuntius, which ſeem to aſſiſt us in affixing a meaning to the word, leſs exceptionable than the one hazarded by Aletes.

* $\frac{16}{26}$ Sept. 1607. † P. 48. ‡ P. 58.

§ Hiſtorie des Mathematiques, vol. II. p. 234.

Obſervations relating to the invention of the teleſcope.

In p. 11. Galilæo ſays " tubum primo plumbeum mihi paravi, in cujus extremitates vitrea duo perſpicilla . . . aptavi." Again in p. 12. he ſays " dum nulla in tubo adeſſent perſpicilla," the rays paſs in one direction, but " appoſitis perſpicillis," they will paſs in another. Hence it appears that Galilæo makes no difference between *perſpicilla* and *vitrea perſpicilla*, but uſes both indiſcriminately for the glaſſes of his teleſcopes. I am inclined indeed to believe that the original meaning of *perſpicillum* was like that of *conſpicilium* a mere lens; that when lenſes were combined in teleſcopes it was uſed both for the glaſs and the inſtrument, until the word *teleſcopium* became common. I am not, however, prepared to bring forward ſufficient authorities for this account of the word, and I have not leiſure at preſent to ſearch for them. It is ſufficient for my purpoſe that we are juſtified in underſtanding the word as meaning a mere lens; for if we conſider it as ſuch in the paſſage under conſideration all the difficulty vaniſhes.—— Kepler complains in this very book *de cometis* [*] of the weakneſs of his ſight; he might, therefore, have probably uſed a ſimple lens to aſſiſt it. Monſieur Dutens [†] has clearly proved that the convex lens was known to the ancients, and at the ſame time [‡] that Borelli calls Zachary Janſens, " conſpiciliorum artifex peritiſſimus," he ſpeaks of his concave as well as convex glaſſes.

There is one circumſtance attending the obſervation itſelf, which ſeems to give ſome ſupport to my interpretation, which is, that it was not made at a time when Kepler was carefully examining the heavens for any new phenomena; but in a walk which he accidentally took with his friend. Now he may eaſily be ſuppoſed to have carried about with him a ſimple lens to correct any fault of his eyes, but it is ſcarcely probable that he would have had with him a teleſcope, made, poſſibly like Galilæo's, of a long leaden pipe. I ſcarcely need add, that whatever clears and ſtrengthens the ſight will diminiſh the apparent diameter of a fixed ſtar, and conſequently increaſe the relative magnitude and light of the comet.

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[*] P. 30. Inſtrumenta Tychonica, &c. &c. et oculos vegetos requirunt, quæ omnia mihi defuerunt, 1607, and in p. 48. Debili ſum viſu, 1618.

[†] Decouvertes des anciens. Partie 3, chap. 10. §. 278. [‡] P. 25.

I am

Obſervations relating to the invention of the teleſcope.

I am conſcious, Sir, that what I now ſend you, is put together in a very haſty manner. Were my time at my own diſpoſal, I would endeavour to make it more fit to meet the public eye; but I am induced to ſend you theſe hints in their preſent ſtate, becauſe I rely as much upon the candour as on the learning and abilities of Sir H. Englefield. If I am miſtaken, he will ſet me right; and if I have any foundation for my opinion, he can eaſily illuſtrate my hypotheſis by the paſſages which agree with it, in the old writers on optics and aſtronomy. I am, Sir,

Your obliged humble ſervant,

E. O.

Oxford, Feb. 10, 1805.

P. S. Since I finiſhed the letter, which I ſent to you on the 10th inſt. I have met with a paſſage, which appears to me to decide the queſtion on which I wrote to you. It is in Kepler's book, which he publiſhed at Francfort in 1604, under the title of *Aſtronomiæ pars optica.* The beginning of it is entitled *Paralipomena in Vitellionem.* He diſcuſſes the nature of the eye, and at p. 178. he begins a ſet of propoſitions on the manner in which it acts as the organ of viſion. The 28th of theſe propoſitions is that which makes moſt immediately for my preſent purpoſe; it is at p. 200, and he enumerates it in the following words.

" Qui remota diſtinctè vident, propinqua confuſè; iis perſpicilla convexa proſunt. Qui vero confuſè vident remota, juvantur concavis perſpicillis."

The mere quotation of theſe words is ſufficient for the proof of what I have aſſerted; but as it may afford ſome amuſement to your readers, I will add his remarks upon them as ſhortly as poſſible.—" Quanta admiratio" he ſays, " rei tantæ tam late propogatum uſum: & tamen cauſum ignorari hactenus." J. B. Porta, he adds, profeſſes to give the reaſon in his optics: but this book he was never able to meet with. Kepler then very ably refutes the opinion of thoſe, who ſuppoſe this effect to be produced by the increaſing or diminiſhing of the apparent magnitude of the object. He offers with conſiderable diffidence his own method of accounting for the phenomenon. He ſuppoſes that in the caſes mentioned in the propoſition the viſion is imperfect, in as much as without the intervention of convex or concave lenſes, the apices of the different cones of rays would fall either before or behind the retina. He ſupports his theory by experiments which he tried on ſhort and long-ſighted perſons; and quotes the paſſage from Ariſtotle, in which he points out the difference between the myops and the preſbytes.

Feb. 12.

II.

Conſtruction of a Line in a Circle, nearly equal to the Side of a Square of the ſame Superficies as that of the Circle itſelf. With Remarks on Pendulums and other Objects. By Mr. J. WHITLEY BOSWELL.

To Mr. NICHOLSON.

SIR, *Feb.* 9, 1805.

Introduction.

HAVING often found a difficulty in computing the preſſure of fluids in tubes and cylindrical veſſels, from all the calculations relative to the gravities of different bodies, which I was acquainted with, being adapted for cubic meaſure, it appeared a very deſirable matter to find an eaſy method of converting round into ſquare meaſure, which induced me at different times to attempt it, and as often to lay it aſide, from not being able to arrive at any ſatisfactory reſult. But lately having had occaſion to reſume this ſubject, I have diſcovered a method of coming ſo very near the truth, that if it ſhould turn out to be in reality not ſo exact as it ſeems to me, it will ſerve ſo well for common computations, and promiſes on that account to be of ſuch utility, that I am induced to ſend it to you for publication, if you approve of it.

To convert round meaſure into ſquare.

As this ſubject has been by many looked on as ſomewhat chimerical, it may be therefore not amiſs to mention, for thoſe of your readers who are not very converſant in geometrical ſtudies, that a mechanical method of computing ſquare meaſure from round is a very ancient and well-known problem; but this method being difficult to apply to the inſide of tubes, and not capable of being uſed in diagrams or drawings, has made another method deſirable: mine for this purpoſe is as follows:—

To

To find a right Line, the Square of which ſhall be equal to a given Circle.

Problem. To find by conſtruction the ſide of a ſquare equal to a circle.

Draw two diameters (I F and C T) biſecting each other at right angles in the center (O) of the given circle; biſect one of the radii ſo formed (O T) in W, and from the extremity (I) of the next adjoining radius, through W draw the right line (I B) to the circumference: This line (I B) is the line required.

Proof.

Proof by trial.

Let a ſquare be formed equal to the given circle by the third corol. prop. fifth, of Archimedes; then take a ſquare formed by the line I B, and place it on this other ſquare, ſo that one angle and the ſide adjacent to it of one, ſhall fall on one angle and the ſide adjacent to that angle of the other; then will it be ſeen that all the other angles and ſides of each will coincide, and the whole of one be equal to the whole of the other.

This kind of proof is nearly the ſame as that of the fourth prop. firſt book of Euclid, on which ſo many other propoſitions depend; and having often tried this method in the above manner, I could never perceive any difference between the two ſquares: In thoſe trials I uſed circles of card paper for the more exact meaſurement of the circumference in the mechanical proceſs directed in the method of Archimedes.*

To the above I have to add the farther proof which follows of the exactneſs of my method, which may make it appear ſtill more certain.

Sometime after I diſcovered the above, looking into a work of the learned Kircher for a ready method of deſcribing a parabola which I knew was inſerted in it, I perceived therein alſo a geometrical method of finding a right line equal to any given portion of the circumference of a circle; and having by this method drawn a right line equal to the fourth of the circumference of a circle, and with it and the diameter formed a parallelogram equal to the circle, and then a ſquare equal to this parallelogram, in the uſual manner: I afterwards formed another ſquare equal to the ſame circle, according to my method before deſcribed, and found no perceivable difference between it and the former ſquare.

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* In order to ſhew how near Mr. B.'s conſtruction approaches to the truth, we may obſerve, that when the diameter is = 1, the area is = 0.7854, and the ſide of the equal ſquare = 0.8862. But in the figure, I W is found by adding the ſquare of the radius to that of the half radius, and extracting the ſquare root; and then by the property of ſimilar triangles, as the radius is to I W, ſo is the diameter to I B, which will be 0.8944 when the diameter is = 1. But this line exceeds 0.8862, or the true ſide of the ſquare, by 0.0082, or nearly one hundredth part. N.

The diſcovery of a fact in geometry often leads to another; one of this kind I have to add here, which is, that a right line (B E) drawn from the extremity B of the line I B, at right angles through the oppoſite diameter (I F) to the circumference, will be equal to a fourth of the circumference.

The problem of Kircher above mentioned, may be found in the *Ars magna Lucis et Umbræ*, prop. 3, lib. 3. pars 2, page 239, and the two following pages.

As the circles which I uſed were under three inches diameter, it is poſſible on ſo ſmall a ſcale a minute error might not be viſible; but even in this caſe it muſt be very inconſiderable.

Properties of pendulums, &c.

I take the opportunity of forwarding the above to ſend an explanation in reply to the annotations on my paper on pendulums, which I hope you will do me the favour to inſert, as you have in ſome degree called on me for it.

I beg leave to obſerve, that in that paper I have endeavoured to put a marked diſtinction (though perhaps not ſufficiently forcible) between the obſervations which admitted of proof, and ſpeculations on new ſubjects: The latter may be conſidered to commence with Huygens' pendulum; and the others on ſimilar principles, along with this, were merely deſigned as objects *propoſed for experiment*, not as things proved, and nothing is inſerted about them but what was intended to be noted as conjectural by the mode of expreſſion.

I conſider it uſeful to propoſe experiments (excluſive of my partiality to this mode of aſcertaining facts), becauſe among your numerous readers there might be ſome who, with little trouble or expence, have opportunities to make trials of this kind, Properties of pendulums, &c.

kind, from the nature of their employments furniſhing them with all the materials neceſſary ready at hand, and who have ſufficient intereſt in the ſubject to induce them to undertake them.

I acknowledge that (as you obſerve) I did not ſtate all my reaſons for thinking that Huygens' circular pendulum, and the others propoſed on the ſame principles, would not be ſo liable to alter their notation of time, as thoſe in common uſe, from a change of temperature; but this was cauſed by the other ſubjects contained in that paper having extended it to ſuch a length as to oblige me to curtail this and other matters, and leave out part of what I had written relative to pyrometers alſo. My reaſon for thinking well of Huygens' pendulum proceeded partly from my reſpect for his opinion, founded on the high idea I have of his mathematical erudition; and becauſe it appeared to me, that the ſize of the circle produced by the gyration of the ball, depending on the impulſe of the moving power more than on the length of the ſuſpending ſtring; that if the ſtring lengthened a little, the circle would not be thereby increaſed; and even if it was increaſed, that the effect of the paraboloidal lamina would prevent its altering the time of each revolution of the ball (granting the theorem of Huygens, which I inſerted, to be true); for though I was aware that the parabola generated would, by the lengthening of the ſtring, become of ſomewhat leſs curvature, yet I thought this change would be ſo minute as not ſenſibly to affect the time, eſpecially as a change apparently greater had not affected it in a ſimilar caſe, which will be mentioned a little farther on, but of this I had doubts then, as appears from what is remarked in page 77, where I mention that I thought the other conſtructions which I propoſed might be better on account of *the ſtring of that of Huygens being liable to lengthen.*

I recommended the other pendulums, on ſimilar principles, for trial, becauſe though the expanſion will increaſe the dimenſions of the parabolical and cycloidal curves, yet ſtill they will not ceaſe to be thoſe curves, which Huygens has proved to have ſuch remarkable properties for the regulation of pendulums; and as it has been found by experience, that a pendulum vibrating in a ſmall arc of a large circle, has the ſame accuracy as when moving in a cycloid, I imagined that there could not at leaſt be more difference between the effects of two cycloids or parabolas a little differing in ſize, than between the cycloid and large circle; and beſides, as the cycloidal cheecks uſed by Huygens (which he found to ſucceed ſo well) were liable to the ſame alteration by expanſion, I looked on his numerous experiments as confirmations of this opinion.

In reply to the obſervation relative to the ſuperior effect of pendulums detached as much as poſſible, I beg leave to obſerve, that a movement interrupted or intermitting (ſuch as more or leſs takes place in clocks with oſcillating pendulums), is ſo very different from one which continues equally without any interruption (as that of the circular pendulum), that it does not ſeem to me concluſive to argue from the effects of one to thoſe of the other.

In the note relative to the circular veſſel encloſing the mercurial tube, I am inclined to imagine you were not aware that this veſſel was directed to be faſtened to the ſpindle ſo as to revolve with it, by which means there would be no lateral motion given to the air but by the friction of the outſide of the caſe; for which reaſon I mentioned that it ſhould be made very ſmooth externally: I alſo directed it to be covered, to prevent the effect of the current of air, which would otherwiſe paſs through it, cauſed by the centrifugal impulſe of the circular motion.

As to what I obſerved about the ſuſpending ſpring, I perhaps expreſſed myſelf too generally: I ſtill think, however, I could point out ſeveral inſtances of gridiron pendulums, where the compenſations were made of one bar againſt another only, without allowing any compenſation for this ſpring.

I think it neceſſary to mention among the emendations of my former paper, that the cycloidal bed of the rolling pendulum, propoſed in it, ſhould be formed of the ſame thickneſs in every part, as it might expand unequally if made thicker in one part than another: As a trial of this ſort of pendulum may be made with a common clock at a ſmall expence, I hope it will induce ſome one better ſkilled in ſuch experiments than I am, to make it, even if the others ſhould not be tried: and as a farther reaſon for expectation of ſucceſs in the trial, it ſhould be conſidered (in addition to what has been advanced in ſupport of the opinion, that the effect of the cycloidal bed would be but little altered by its expanſion), that, Properties of pendulums, &c.

that, on account of the ſhortneſs of this bed, it could not poſſibly expand as much in length as the 39 inches of wire contained in a common pendulum-rod; and that, as the greateſt expanſion of the wire is certainly in the direction the moſt injurious to the effect of the pendulum, which is not the caſe with that of the cycloidal bed, it is probable that the rolling pendulum would be found ſuperior to that in common uſe.

With regard to the obſervations on pyrometers, I own I never ſaw that of Deluc; but as your deſcription of it is not ſufficiently minute to enable me to perceive how the ſtandard in it for meaſuring the different bars, could eſcape expanſion in an increaſe of temperature of the atmoſphere, I ſtill can conceive no method by which the ſtandard for meaſurement in any pyrometer, could be prevented from altering its length from the above cauſe, but by keeping it artificially at a fixed temperature, by ſome means diſtinct from the apparatus, ſimilar to thoſe of Mr. Ramſden, which you have mentioned. What I wrote on this ſubject was intended as a caution in future experiments, and if it has produced the more extended publication of means to avoid the error I pointed out, or ſhall hereafter do ſo, my view will be fully anſwered.

Your very humble ſervant,

J. WHITLEY BOSWELL.

III.

Some Remarks upon the Experiments by which Mr. Ez. WALKER *has endeavoured to explain the apparent Enlargement of the Moon near the Horizon: with a Statement of ſome Facts upon which that Phenomenon ſeems chiefly to depend. In a Letter from* C. H.

To Mr. NICHOLSON.

SIR,

Mr. Walker's experiments adverted to.

I SHOULD, with your permiſſion, be glad to point out to your correſpondent, Mr. Ezekiel Walker, a theorem in optics, to which, it appears to me, that he has not paid ſufficient attention in the courſe of his ſtudies.

Rays

Optical theorem. The focus of a lens is ſhorter the larger the aperture, and the image of courſe ſmaller.

Rays from a diſtant object which paſs through a convex lens near its center, will have their focus more diſtant from the lens, than thoſe rays which paſs through it nearer to its circumference; and, conſequently, the image formed by the central rays, will be as much larger than that formed by the rays of the circumference, as the focal diſtance of the former ſhall exceed that of the latter. With a given aperture, the focus will be that point in which the greateſt number of rays coincide, for, *there*, will the image appear moſt diſtinct; and, conſequently, as we enlarge the aperture of the lens, the central rays muſt be combined with others more convergent, the new focus will approach the lens, and the image be, of courſe, proportionably diminiſhed.

This is contrary to Mr. Walker's reſult; in which he was probably deceived by the differences in brightneſs.

Propoſal that he ſhould vary and repeat the experiment.

The total diſagreement of this *theorem* with the reſult of Mr. W.'s experiments, I can only account for by ſuppoſing this gentleman, while he enlarged the aperture of his lens, not to have paid attention to the increaſing brightneſs of the image, which, by making a ſtronger impreſſion on the retina, would appear to increaſe in magnitude with every enlargement of the aperture. If I might be allowed to offer my advice to Mr. W. I ſhould recommend him to repeat his experiments, and ſubſtitute, for the flame of the candle, a ſkreen having a circular opening covered with thin paper. This, with a light behind it, may repreſent the horizontal moon. Some characters ſhould be marked upon it, in order to determine, with more facility, when the image is moſt diſtinct. The precaution of reducing the different images, as nearly as poſſible, to the ſame degree of brightneſs previous to meaſurement, muſt not be neglected. I am ſurprized indeed, that the ſtructure of the eye, which Mr. W. profeſſes to have kept in view in the courſe of theſe experiments, did not point out to him the importance of this circumſtance, which is ſo admirably anſwered, in the natural organ, by the contraction and expanſion of the pupil.

Phenomenon of the horizontal moon occaſioned in part by the ſuppoſition of greater diſtance, by the aerial perſpective, and other circumſtances;

As the phenomenon of the horizontal moon has of late much engaged the attention of ſeveral of your correſpondents, I will avail myſelf of the preſent opportunity to add a few remarks on that ſubject. It appears, at firſt view, that the moſt obvious method of accounting for the phenomenon, is the following: We believe the moon more diſtant from us when in the horizon than when in the meridian; but we ſee it, in both caſes, under the ſame apparent angle, and, according to our uſual habits of judging of the magnitudes of objects, neceſſarily refer the idea of greater magnitude to the greater diſtance. The aerial perſpective, and various other circumſtances, which combine to make the horizontal diſtance appear the greateſt, have been ſo well and ſo often deſcribed, that it would be ſuperfluous to repeat them here. I muſt obſerve, however, that this appearance of greater diſtance in the horizontal moon, though the firſt and moſt obvious circumſtance which preſents itſelf to explain the illuſion, is not, in my opinion, that which has the greateſt ſhare in producing it.

but theſe have not the greateſt ſhare in producing it.

After having conſidered the ſubject with much attention, I am of opinion that the chief ſtreſs ought to be laid on the following particulars:

New explanation. The moon is rarely ſeen in the horizon.

Small objects eclipſe the moon at conſiderable altitudes.

The elevated clouds are alſo large, becauſe near; and the moon ſeems ſmall, becauſe compared with them. But in or near the horizon, the clouds, buildings, and other large objects, will appear ſmall, and hide but little of the moon's diſc; which will therefore, by compariſon, be ſeen on a grand and enlarged ſcale.

We may be ſaid rarely to ſee the moon in the horizon. Our habitual acquaintance with it is at ſome conſiderable altitude. Hence it follows, that all the objects with which it comes in apparent contact, and with which we occaſionally compare its diſk, are near to us, and ſubtend angles proportionably large. Thus, for inſtance, we often ſee the moon loſe a large portion of her face behind the branch of a tree or a weather-cock, and totally diſappear behind a chimney. The clouds, too, which fleet before her, appear, from their proximity to us, on a gigantic ſcale. Theſe are the circumſtances under which we have inſenſibly formed our general ideas of the moon's magnitude. Let us now ſurvey it in the horizon. The caſe is widely altered. We have now an opportunity of comparing it with various large objects, which perhaps, being themſelves on the verge of a diſtant horizon, will be reduced to very trifling dimenſions; and thus the caſtle and maſſy cathedral will hide but a ſmall part of that globe, which we have commonly ſeen half eclipſed by a ſpout or a weathercock. The clouds too, reduced by diſtance, appear on a much ſmaller ſcale when compared with the moon. In ſhort, it is no longer the ſame moon we were acquainted with in the meridian, but a much larger and more majeſtic ſphere, the novel appearance of which ſtrikes moſt of its ſpectators with a degree of awe and amazement.

Theſe are the principal circumſtances.

Though it appears to me that the circumſtance I have pointed out, of our habitual compariſon of the moon with objects of known magnitudes, has the greateſt ſhare in producing the illuſion by which it appears largeſt in the horizon, I would by no means be underſtood to reject the principal circumſtances, long ſince noticed as contributing towards the ſame effect. Different circumſtances will neceſſarily have more or leſs influence with different perſons, according to the tenor of their previouſly acquired habits and obſervations.

I remain, Sir,
Your obedient ſervant,
C. H.

Taviſtock Place, Feb. 15.

ANNOTATION. W. N.

THIS very ſtriking and perſpicuous explanation of the enlargement of the moon near the horizon, will bring to the reader's recollection a great number of facts, where the abſolute magnitudes of objects, of which the diſtances are imperfectly known, are erroneouſly eſtimated by compariſon with other objects lying in the ſame direction. Navigators muſt have been often ſtruck with the extreme minuteneſs of the image of the ſun or moon, when brought down by Hadley's quadrant ſo as to be ſeen projected upon the near objects below the horizon. Since the peruſal of this communication, I have repeated the experiment on thoſe luminaries. If the image of the moon be removed a certain number of degrees from the direct ray, it is eaſy, by altering the poſition of the quadrant, to obſerve that image in the horizon to the right or left, or upon the pavement before us, or at a great elevation in the heavens. In theſe caſes its apparent magnitude, while the angular ſize and brightneſs remain unaltered, is found to be ſmall at the great elevation, large in the horizon, and moſt ſo when projected upon diſtant objects, and it becomes a minute ſpeck when ſeen depreſſed among the objects cloſe to the obſerver. The ſame experiment may be made with a pane of glaſs, but leſs objectionably, becauſe it might be urged that the quantity of illumination is different according to the obliquity of reflection.

Experiment with Hadley's quadrant, in which the ſun and moon appear of different magnitudes, according to the objects on which their images are projected.

IV.

A Mathematical Theory of the Speaking Trumpet. By JOHN GOUGH, *Eſq.*

To Mr. NICHOLSON.

SIR,

Theory of the ſpeaking trumpet.

THE ſpeaking trumpet is a conical tube, which receives the human voice from the mouth, and encreaſes the range of it. The ſtructure of theſe inſtruments, and the materials of which they commonly conſiſt, teach us to refer this ſingular faculty to two cauſes. In the firſt place, the æriel pulſes undergo certain modifications in the cavity of the trumpet; the nature and effects of which I am going to inveſtigate. Secondly, the metallic ſhell of the inſtrument augments the power of the voice, by its aptitude to conduct thoſe impulſes; which are impreſſed upon it, partly by the pulſes of the included air, and partly by the direct action of the Larynx, tranſmitted through the medium of the face to the mouth-piece of the inſtrument. Before I enter upon the inveſtigation of the effects reſulting from the firſt cauſe, it will be neceſſary to premiſe the following lemma:

Article 1. Let O R S r (*Fig.* IV. *Plate* 7) be an evaneſcent ſphere of air, which is agitated by two or more pulſes, beginning and ending together, and radiating from the points P p, &c. the vibatory motion of O R S r, in any direction whatever is equal to the joint ſum of the forces, which the pulſes would impreſs upon it ſeparately. Join P O, O p; and draw the lines R O r, S O s perpendicular to P O, Op; now as theſe lines are evaneſcent, every point of each of them is equally preſſed at any inſtant by its reſpective pulſe. But the force imparted to R O r by P, agitates the ſphere O R S r equally in all directions, *Principia*, 422; the ſame holds true of the force of p upon S O s. Now theſe forces cannot coaleſce or be compounded ſo as to act in a ſingle direction,—*Manch. Mem.* V. 5. p. 660; conſequently they act with their joint powers in all directions.

Art. 2. This being premiſed, let the fruſtum A B C D (*Fig.* 3.) repreſent a trumpet, the ſhell of which is a non-conductor of ſound. After a pulſe of the voice has paſſed through the aperture

Theory of the ſpeaking trumpet.

aperture A B, it is bounded in front by a ſpherical ſurface, having O, the vertex of the cone D O C, for its centre, and ſubtending the plane angle A O B; in other words, let ſuch a ſurface be deſcribed any where in the fruſtum A B C D, and the variable denſity of any one particle of air in contact with it, will be equal to the cotemporary denſity of any other particle in the ſame ſurface. For as ſoon as the motion is propagated into the internal air, the pulſe will expand as far as the ſides of the tube will permit: *Princip.* 42.2: after which, the motion will be continued to the oppoſite aperture C D, in right angles, 43 *ibid.* therefore it will be propagated along the lines A D, B C with equal celerities, ſuppoſe to T and V.—On the plane T V deſcribe the ſurface T N V, in which the denſity of the air is every where equal at any inſtant: Now every point of ſuch a ſurface acts upon the air externally in contact with it, and is re-acted upon by the ſame perpendicularly to itſelf, in ſuch a manner that equal portions of the ſurface ſupport equal degrees of preſſure. Let Uu be an evaneſcent particle of the curve T N V, draw U K, uk perpendicular to N O; then the ſuperficial ring between the planes U K, uk is as $UK \times Uu$; but this ring is as the preſſure upon it; which is alſo inverſely as the radius of curvature at U, therefore if $UK \times Uu$ be conſtant, the radius of curvature is conſtant; conſequently it is equal to O T, and the truth of the propoſition is manifeſt.

Art. 3. Let f, be the force of a pulſe in the aperture A B; and let the internal air of the cone be agitated with an equal and ſimilar impulſe at the point O; then the effect thus produced in the trumpet, will be the ſame with that of the voice acting at A B, (by *Art.* 2.) But had the cone been out of the way, the ſame force at the diſtance O A, would have been uniformly diffuſed over the ſurface of the hemiſphere, having O for its centre.

Art. 4. Put $AO=r$; the variable diſtance $OT=x$; the radius of an evaneſcent circle$=d$; the verſed ſine of half the angle $AOB=p$; $3.1416=c$. Then as the force f is diffuſed over the hemiſphere whoſe radius$=r$, in free air by *Art.* 3. that part of it which reſides in an evaneſcent point of the ſame ſurface$=\frac{fd^2}{2r^2}$.

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Art. 5. The magnitude of the ſurface T N V $= 2cpx^2$: but the force f is diffuſed over this ſegment by *Art.* 3. conſequently that part of it which is confined to an evaneſcent ſpace $q = \frac{fq}{2cpx^2}$

Art. 6. Since the ſpherical ſurface T N V is convex to the plane C N D (*Art.* 2.) the agitation of the air in the latter will commence at the centre N, and extend thence by the application of ſucceſſive circles of the ſphere T N V to equal circles of the plane C D, having N for their centre; which operation will continue until T and V fall upon C and D.—Now as C N D is in contact with unconfined air, each point of it, upon being ſtruck, will become a centre, from which a pulſe will radiate freely. (*Princip.* 42.2.)

Art. 7. Let P be ſuch a phyſical point, and let its area $= q$; then if an evaneſcent plane $= q$ be drawn through P perpendicular to O P or x, the force impreſſed upon it by the trumpet will be $\frac{fq}{2cpx^2}$ (*Art.* 5) but the ſame force is imparted at the ſame time to the equal phyſical point P in the plane (*Art.* 1.)

Art. 8. Let L be the place of an ear, in O N produced, or more properly of a minute ſphere of air. Put N P $= y$; P L $= w$. Now the pulſe proceeding from P will have due effect upon the point L (*Manch. Mem.* v. V. p. 662. cor. 1); which effect is as $\frac{fq}{2cpx^2w^2}$ (*Arts.* 6 and 7): hence we have (by *Art.* 4) as $\frac{f}{r^2} : \frac{fd^2}{2r^2} :: \frac{fq}{2cpx^2w^2} : \frac{fqd^2}{4cpx^2w^2}$, for the force acting upon the ſphere L having d for its radius. But the number of points which act together $= \frac{2cyy'}{q}$ (*Art.* 6.); conſequently their united forces $= \frac{2fd^2yy'}{4px^2w^2}$. (*Art.* 1.)

Art. 9. Put L N $= g$; N O $= e$; O C $= a$; C L $= b$; and the correct fluent of the preceding expreſſion is $\frac{fd^2}{4 \times \overline{pg^2 - pe^2}}$ drawn into the hyp. log. of $\frac{ga}{eb}$; when g and e are unequal. But if C D biſect O L; put C N $= k$, and the correct fluent becomes $\frac{fd^2k^2}{4pg^2b^2}$.

Art. 10.

Theory of the ſpeaking trumpet.

Art. 10. Though the foregoing fluents have received one correction, each of them requires a ſecond, ariſing from certain circumſtances peculiar to this problem. For it is evident, that the commencement of the agitation at N is prior to that at C, by a part of time, which is as C O − O N; conſequently the completion of the former pulſe at L, will precede the termination of the latter at the ſame point by a ſimilar interval; which is as L C + C O − O L. Put this time $= t$, and the duration of one vibration of the larynx $= l$; alſo let the indiviſable interval, or $\frac{1}{10}$ of a ſecond according to Euler be denoted by m; then multiply the expreſſions in *Art.* 9 by the fraction $\frac{m-t}{l}$.

Art. 11. The force determined in the preceding paragraphs, is that which a non-conducting tube imparts to an evaneſcent ſphere of air. This circumſtance affords a plauſible objection to the preſent theory; becauſe the ſenſe of hearing is not confined to a phyſical point. But as the ſeat of this ſenſation is of a given extent, at leaſt in each perſon, the number of theſe vibrating points, in contact with the ſenſitive ſurface, is alſo given: from this it follows, that the effect of one particle is nearly a true meaſure of the power of the whole number, when the ear lies at a great diſtance from the plane C N D, repreſenting the trumpet's mouth.

Art. 12. So much of the preſent ſheet is occupied by the conſideration of the firſt cauſe, that my remarks on the ſecond, or the effects of the metallic ſhell, muſt be confined within narrow limits; theſe I ſhall begin as follows.

Art. 13. Since the conducting power of the trumpet is given, the vibrations communicated to the ſmall end of it by the breath and face, will paſs to the oppoſite extremity in a given time; which call L. Alſo let h be the force of a vibration at the diſtance 1; and C L or b expreſs the ſpace betwixt the trumpet and a remote ear.

Art. 14. In the firſt place, let l be equal to L; then a ſingle pulſe of the trumpet will ſtrike the ear for every vibration of the voice; and the force of it will be $\frac{h\,m\,d^2}{L\,b^2}$ by my former paper on this ſubject. In the next place, let l be an aliquot part of L; then the number of pulſes impreſſed on the ear in the

Theory of the ſpeaking trumpet. the time of L is $\frac{L}{l}$; hence the force $\frac{h\,m\,d^{n}}{l\,b^{n}}$. Add this equation to the former of thoſe found in *Art.* 9; and the ſum gives the force of the trumpet upon a diſtant ear.

Art. 15. But if l be an aliquant part of L, the ſound will be imperfectly formed; becauſe the quantity $\frac{L}{l}$ being a mixed number, the vibrations of the larynx will be conducted along the metal, and diſcharged into the air in a diſorderly manner, ſo as to deſtroy the continuity of the note. The ſame cauſe, in all probability, conſtitutes the difficulty which the inexperienced find in their firſt attempts to ſound horns and military trumpets: for the lips, which are compreſſed by the end of the tube, form the mouth piece in this caſe; and the art of producing the ſound ſeems to conſiſt in cauſing the edges of them to vibrate in aliquot parts of L, or the interval belonging to the loweſt note of the inſtrument. The ſame origin may alſo be aſcribed to the muſical ſcale, compoſed of what are called the trumpet notes; the intervals of which decreaſe in the following ratios, 1, $\frac{1}{2}$, $\frac{1}{3}$, $\frac{1}{4}$, $\frac{1}{5}$, &c. for $\frac{1}{2}$ is the greateſt aliquot part of unity, $\frac{1}{3}$ the next in ſucceſſion, and ſo on.

Art. 16. If the remarks contained in the two preceding paragraphs be juſt, the perſon who makes uſe of a ſpeaking trumpet, muſt attend to the management of his voice; the pitch of which ought to coincide with ſome note in the trumpet ſcale of his inſtrument: for proper attention to this circumſtance will make the ſound of the metallic ſhell perfect, and bring it into uniſon with the other ſound; which ariſes from the aerial pulſes, paſſing through the cavity of the tube, at the ſame inſtant. In this manner, the greateſt ſound will be produced, ſuppoſing the power of the voice to be conſtant; becauſe the two ſets of pulſes, conſtituting the uniſon, will impreſs the greateſt poſſible force upon the ear in a given time. On the other hand, if the foregoing precaution be neglected, the imperfect ſound of the metal will diſturb the uniformity of the other, which is formed in the cavity; and the force impreſſed on the ear by ſo diſorderly an aggregate of pulſes, will be interrupted and inconſtant.

Art. 17. Perhaps it is unneceſſary to remark, that the perfect elaſticity of the air forms the baſis of the preſent theory; a ſuppoſition which in all probability is not altogether juſt; but, when the ſmallneſs of the angle O L C is recollected, the error ariſing from the hypotheſis vaniſhes in a great meaſure.

JOHN GOUGH.

Middleſhaw, Feb. 8, 1805.

V.

Obſervations on Baſalt, and on the Tranſition from the vitreous to the ſtony Texture, which occurs in the gradual Refrigeration of melted Baſalt; with ſome geological Remarks. In a Letter from GREGORY WATT, *Eſq. to the Right Hon.* CHARLES GREVILLE, *V. P. R. S. From the Philoſophical Tranſactions for* 1804, *p.* 279.

(Concluded from Page 126.)

Fuſion and cooling may produce different reſults in compound bodies, the more powerful the one and gradual the other.

A CURIOUS diverſity may prevail in the products of a compound body ſubjected to fuſion, when abſolute ſolution is produced. When merely ſimple fuſion takes place, the aggregation of the parts only is deſtroyed: the fluidity ariſes from the facility with which they move on each other; and a regulated diminution of temperature, by facilitating their re-union, can hardly fail to recompoſe the ſame ſpecies that formerly appeared to exiſt in the compound. But, if the molecules have been diſſolved and decompoſed, and their component particles diffuſed through the fluid, there ſeems to be very little probability that any re-union ſhould compoſe the ſame molecules. It is more likely that new compounds will be formed, from which new molecules, and of courſe new cryſtals, will be generated; and that, conſequently, the ſame rock may become the parent of very diverſified offspring. Theſe will however retain ſome traces of their origin; for, as there can be no fuſion of a compound body imagined, in which the mutual action of the components will not decompoſe ſome portion, there can be no ſolution ſuppoſed ſo perfect that every molecule ſhall be deſtroyed. In the firſt caſe, there will exiſt the germs of a new compoſition; and, in the ſecond, there will remain the relics of the old.

The aqueous formation exhibits ſimilar facts.

If theſe obſervations are correct, conſiderable utility ſeems derivable from them, in the explanation of ſome geological problems.

reous ſta-
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, then
larly ſpa-
then ſpar.

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problems. It will appear, that they ſtrikingly illuſtrate the analogy which exiſts between the aqueous and igneous formations, and ſhow that preciſely the ſame order and kind of arrangement is followed, in the generation of ſtony maſſes from water as from fire; for, the change of ſtructure, which I have obſerved to be the moſt inexplicable part of the proceſs by which glaſs paſſes into ſtone, is almoſt exactly imitated in the formation of calcareous ſtalactites. Succeſſive depoſitions of calcareous carbonate, form a ſtalactite, which at firſt is fibrous. A continuance of the proceſs cauſes the fibrous ſtructure to diſappear, and the ſtalactite becomes irregularly ſpathoſe. The irregularities then vaniſh, and it becomes perfect calcareous ſpar, diviſible into large rhomboids, with the form peculiar to that mineral; and all the gradations may be found in the ſame ſpecimen. Nor is this change confined to a few ſolitary ſpecimens; for a conſiderable extent of coaſt near Sunderland, is formed of a lime-ſtone compoſed of radiated ſpheroids, from half an inch to three inches diameter, imperfectly united. When one of theſe ſpheroids attains ſomething more than the uſual magnitude, it becomes compact in the heart; and it is not unuſual to diſcover portions of the rock, in which the radii have entirely diſappeared, and the whole maſs has become compact. It is probable that the entire formation of oolithi and piſolithi is owing to the ſame cauſe; and that they are prevented from ever arriving at great ſize, by the union of their ſurfaces, and their ſubſequent conſolidation into compact limeſtone, into which they are continually found to graduate.

Petroſilex, of doubtful origin, ſimilarly arranged in its ſtructure.

Hitherto, I have ſelected inſtances from ſubſtances which have an undiſputed claim to an aqueous origin. I ſhall now, on the authority of Dolomieu, inſtance a ſimilar arrangement, in a ſubſtance reſpecting the origin of which theoriſts are not agreed. A ſpecies of petroſilex is found in the Val de Nido, in Corſica, which contains radiated petroſiliceous glands, from half a line to an inch in diameter. Theſe glands only differ from the baſis by their radiated ſtructure, and their colour; and appear to indicate very clearly, that the rock was ſubjected to a ſpecies of arrangement which, if it had been completed, would have changed its nature, and probably would have rendered it porphyritic; for Dolomieu obſerves, that the centre of the glands was often occupied by a ſmall cryſtal of feldſpar.* The extra-

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* Dolomieu. Journal de Phyſique, 1794, page 269.

Globular granite at Corſica.

ordinary rock called the globular Granite of Corſica, is an analogous inſtance. It is compoſed of cryſtals of hornblende, feldſpar, quartz, and mica, in confuſed aggregation; and in this baſis are immerſed ſpheroids, about an inch and a half or two inches in diameter, compoſed of concentric alternate coats of quartz and hornblende. The centre is principally occupied by hornblende; this is ſurrounded by a zone of quartz. Theſe ſpheroids are radiated to the centre. There can be little doubt that this rock is merely the reſult of interrupted cryſtallization; and that, if the proceſs of arrangement had continued, this ſtructure would have diſappeared, and the whole rock would have reſembled the preſent baſis. Hitherto, this very ſingular rock has only been found in detached fragments.*

Solution not being indiſpenſable to cryſtallization, but only mechanical ſuſpenſion, many geological difficulties are removed.

The admiſſion that ſolution is not a requiſite of cryſtallization, appears to me an important conceſſion in favour of the aqueous ſyſtem, which has laboured under very great embarraſſment, from the difficulty of diſſolving quartz. If a very perfect mechanical ſuſpenſion be all that is requiſite, we may ceaſe to wonder at the almoſt daily formation of petrified wood, (in which, though cryſtallization does not actually take place, a very perfect arrangement is indicated, by the intimate union of the ſiliceous particles,) or of hydrophanous ſemi-opals in the decompoſed ſerpentine of Muſſinet, near Turin, or of chalcedony containing drops of water, in the decompoſed baſalt of Vicenza.

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* I ſhall venture to quote another inſtance, on the authority of Profeſſor Playfair. "The ſalt rock in Cheſhire, which lies in thick beds, interpoſed between ſtrata of an argillaceous or marly ſtone, and is itſelf mixed with a conſiderable portion of the ſame earth, exhibits a very great peculiarity in its ſtructure. Though it forms a maſs extremely compact, the ſalt is found to be arranged in round maſſes, of five or ſix feet in diameter, not truly ſpherical, but each compreſſed by thoſe that ſurround it, ſo as to have the ſhape of an irregular polyhedron; theſe are formed of concentric coats, diſtinguiſhable from each other by their colour, that is, probably, by the greater or leſs quantity of earth which they contain; ſo that the roof of the mine, as it exhibits a horizontal ſection of them, is divided into polygonal figures, each with a multitude of polygons without it, having altogether no inconſiderable reſemblance to a Moſaic pavement. In the triangular ſpaces without the polygons, the ſalt is in coats, parallel to the ſides of the polygons." Illuſtration of the Huttonian Theory, page 37.

I am informed, that the ſiliceous depoſition at Geyſer, is at firſt a porous friable maſs, and that the addition of more molecules renders it fibrous; alſo that, on a farther addition, the fibrous ſtructure diſappears, and the whole aſſumes the compact even texture of chalcedony or flint. If I am not miſinformed, a ſeries of ſpecimens illuſtrating this tranſition, exiſted in the cabinet of the late Dr. Hutton, of Edinburgh.

Since the cryſtallizing may not take place in the order of fuſibility, refractory ſubſtances may be impreſſed by ſuch as are more fuſible.

I have endeavoured to ſhow, that in the cryſtallizations reſulting from igneous fuſion, it is not only poſſible but probable, that the moſt infuſible ſubſtances might not be the firſt to cryſtallize; and this appears to involve important conſequences, for it partly removes one of the greateſt difficulties that embarraſſes the igneous theory, by explaining the poſſibility of refractory ſubſtances generated by fire being impreſſed by the forms of more fuſible ones. It ſeems, however, that the ſame order of arrangement would prevail in ſubſtances that were ſuſpended in a fluid medium, as the degrees of attraction would be the ſame. In either caſe, the firſt ſtep by which the arrangement of an apparently homogenous maſs commenced, would probably be the accumulation of particular molecules into little globules. Such ſeems to have happened in variolites, and other rocks which contain ſpherical concretions of a different nature from their baſis. Still farther advanced is the arrangement of porphyries: the molecules of one ſpecies have aſſumed a regular cryſtalline form; and ſometimes two or even more varieties o cryſtals are formed, which remain unmixed in the unarranged baſis. If the remaining molecules of that baſis are ſuſceptible of cryſtallization, it may be fairly concluded, that an extenſion of the proceſs of arrangement would convert the porphyry into granite, or at leaſt into one of the compound aggregates of cryſtals which conſtitute the numerous tribes of granites, grunſteins, and ſienites; and it ſeems equally probable that this might be accompliſhed, whether the molecules were indebted to a ſuitable temperature, or to an aqueous medium, for the requiſite facility of movement.

Inſtances and remarks.

Granites and porphyry.

The formation of granite and other rocks, muſt however be referred to the ultimate perfection of cryſtallization, by which all the molecules have been permitted to arrange. Thoſe granites called porphyritic, in which large cryſtals of feldſpar are imbedded in a baſis compounded of the ordinary ingredients of granite in ſmall grains, are apparently generated from a menſtruum in which the molecules of one ſpecies, being greatly predominant in number to the reſt, are the firſt to exerciſe their polarity, and conſtitute large cryſtals, which are afterwards ſurrounded by ſmaller ones, reſulting from the ſucceſſive ſeparations of the remaining elementary molecules.

The paſſage from the vitreous to the ſtony ſtate, from that to the porphyritic, and laſtly to the granitic ſtate are obſervable in lavas.

The changes of the ſubſtance that led to the foregoing remarks, ſerve to ſhow that they are not altogether hypothetical; and any proof that may appear deficient, ſeems to be provided by the phenomena exhibited by lavas, in which may be obſerved every ſtep of the paſſage from the vitreous to the ſtony, from that to the porphyritic, and finally to the granitic ſtate. The lava of Lipari, which paſſes from glaſs to lava, by the generation of minute globules, may be cited, on the authority of Spallanzani, as an inſtance of the commencement of the proceſs of arrangement;[*] and, were not their origin ſtill diſputed, I might alſo cite the pitchſtone lavas of the Euganean hills. It would appear, that the tranſition from the ſtony to the porphyrytic ſtate is rapid, for perfectly homogeneous lavas are among the rareſt of volcanic products. The porphyritic lavas are moſt numerous; and it is needleſs to detail the varieties they preſent.

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[*] Spallanzani, *Viaggi alle due Sicilie. Tomo Secondo*, page 238. The whole paſſage, literally tranſlated, ſtands thus: "This lava has a baſis of feldſpar, of a fine and compact grain, a ſplintery fracture, rough to the touch, and emitting ſparks, like flint, when ſtruck with ſteel. It has an aſh colour, in ſome places approaching to a leaden colour. It is thickly filled with an immenſity of little bodies, which would be diſtinguiſhed with difficulty, from the reſemblance of their colour to that of their baſis, were it not for their globular form. But this lava is joined to a great maſs of glaſs, which forms a whole with it, without any diviſion or ſeparation between them; and this lava, which in many places retains its own nature, is in many other places reduced to glaſs. Some parts of this glaſs are filled with the ſame little bodies, but other parts are pure glaſs. This is in general very compact, has a dead black colour, and breaks rather into irregular pieces than into undulated fragments, as glaſs properly does. Beſides, it has I know not what of unctuoſity to the touch, and to the eye, which is not perceptible to the more perfect volcanic glaſſes. It yields ſparks with ſteel, like the lava; but the lava is wholly opaque, and the glaſs, at the angles and thin edges, has conſiderable tranſparency. It is only opaque where the globules are, which appear to be particles of lava."

But, though the proceſs of arrangement has often only advanced thus far, it has in many inſtances proceeded much farther, and it is by no means unuſual to find the entire baſis regularly arranged into cryſtalline bodies; thus, to cite a well known inſtance, in many of the ancient lavas of Somma, large augites are imbedded in a cryſtalline maſs, formed of minute cryſtals of leucite, together with another cryſtalline ſubſtance, whoſe nature is not perfectly determined.

Volcanic glaſs.

The caſual occurrence of volcanic glaſs is nowiſe at variance with this account, as it is ſufficiently probable, that ſome glaſſes may have a much greater tendency to cryſtalline arrangement than others poſſeſs; and it cannot appear extraordinary, that regular cryſtals ſhould ſometimes be generated, even in the glaſs, as it is a matter of daily occurrence in artificial glaſſes, and in furnace ſlags.

Peculiar bodies may be afforded by this method of ſeparation.

If the diſtinction attempted to be ſhown between igneous fuſion and ſolution be eſtabliſhed, it may offer a means of accounting for the abundance of peculiar bodies in lava, which do not exiſt in other ſituations, or at leaſt are of extremely rare occurrence. For, if the igneous action decompoſes the molecules of the ſubſtances on which it operates, there ſeems every probability that new compounds may reſult, diſſimilar to any ſubſtances we are acquainted with. It would appear, that the neceſſity of imagining an undiſcovered ſtratum abounding in leucites, chryſolites, and augites, may be diſpenſed with; and, as I have endeavoured to ſhow the probability that the moſt infuſible ſubſtances will not be the firſt to cryſtallize, the penetration of refractory leucites by fuſible augites, will ceaſe to be an argument againſt both being generated in the lava. I may alſo obſerve, that the ſame cauſes which vary the cryſtallized bodies reſulting from igneous ſolution, muſt operate upon the unarranged baſis; and that the ſame rock may be fuſed into lavas extremely diſſimilar, as their varieties muſt depend on the degree of ſolution which the fuſion has accompliſhed.*

The facts and doctrines here given will not affect the queſtion of the igneous or aqueous formation of baſalt.

If the analogy attempted to be ſhown between the aqueous and igneous formation appear founded, the tranſition from glaſs to ſtone can no way affect the great queſtion which has ſo long divided geologiſts, about the origin of baſalt; for though it is ſynthetically demonſtrated that baſalt may be formed by fire, the converſe of that propoſition ſtands ſupported by ſtrong analogical arguments, and its formation by water muſt be allowed to be at leaſt equally poſſible. How far the probabilities derived from the examination of baſaltic formations may influence the ultimate deciſion, is an enquiry in which I ſhall not now engage; though I cannot avoid recalling to my mind, the numerous inſtances of petrifactions found in baſalt, and, as a counterpoiſe to that obſervation, the equally numerous inſtances in which the heat emanating from it appears to have indurated ſtrata, and coaked beds of coal. One remark may be ſtated here with propriety, as it ariſes immediately from the experiment which has occaſioned theſe obſervations. In the ultimate reſult of that experiment, the arrangement of the molecules was much more perfect than in the original rock. It might be ſuppoſed, that a longer continuance of the ſuitable temperature was afforded it. This, however, could not be, for the maſs was only a few feet long, and a few inches thick; the fire was only maintained a day; and the whole was cooled in a week. But the hill of ſolid baſalt, from which the ſubſtance operated upon was taken, is ſeveral miles long, and ſeveral hundred feet high; and, ſuppoſing it to have been irrupted in a ſtate of igneous fuſion, it muſt have required months, nay years, for its refrigeration. How then comes it, that the proceſs of cryſtalliſation is ſo little advanced? How comes the confuſion of its texture to indicate the very reverſe of the tranquillity and perfection of arrangement, which may be fairly aſſumed as neceſſarily attending the extremely gradual changes of ſo immenſe a maſs?

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* The evidence of the generation of leucites in the lava which contains them, collected by Leopold de Buch, and Breiſlac, and finally acquieſced in by Dolomieu, appears ſo ſatisfactory, that it can hardly be deemed preſumptuous to aſſume the point as determined

Why ſo little of cryſtallization appears in the ſtructure of baſalt.

This objection admits of being obviated, upon the ſuppoſition that, in the proceſs of melting, the molecules of the baſalt were decompoſed; and that the new ones generated were more diſpoſed to cryſtallize than thoſe whoſe place they ſupplied. This explanation is in ſome degree juſtified, by the

Probably becauſe new molecules were formed more diſpoſed to cryſtalliſe.

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mined. Neither de Buch nor Dolomieu have been able to convince themſelves that the augites were alſo formed in the lava; but I confeſs myſelf entirely unable to appreciate the cogency of their arguments, which ſeem annihilated by the admiſſion they have made in favour of leucites.

total diſappearance of the minute fieldſpar and hornblende of of the baſalt; inſtead of which, the regenerated ſtone contains thin laminæ of cryſtals, which are probably augites.

The diviſion of baſalt into globular maſſes during its decompoſition, indicates the formation of radiated ſpheroids therein;

I cannot leave this ſubject without noticing ſome particulars, in which the proceſs of arrangement deſcribed in the early part of this letter, appears to yield a probable explanation of ſome of the peculiarities of baſalt. The general diſpoſition of baſalt to divide into globular maſſes, in decompoſing, is too remarkable a fact to have eſcaped the attention of naturaliſts; though, as far as I am informed, no ſatisfactory explication of it has been given. The common effects of decompoſition are obviouſly inadequate; for it is common to ſee a large block of amorphus baſalt ſeparate into numerous balls, after a few months or years expoſure to the weather; and, rapid as the proceſs of decompoſition has been in the intervening portions, theſe balls reſiſt its father progreſs with uncommon obſtinacy. May not this be attributed to the formation of the radiated ſpheroids, whoſe occurrence in my experiment I have already mentioned? and may not their greater reſiſtance of weather ſimply ariſe from their aggregation being more perfect than that of the incoherent molecules which have filled the intervals between them? Though the radiated ſtructure has diſappeared to the eye, theſe portions of the ſtone retain the ſuperiority of more perfect internal arrangement; and, if my pigmy experiments could yield ſpheroids of two inches diameter, there can be no difficulty in ſuppoſing that the grand operations of nature may produce them of ſeveral feet. The ſeparation of the decompoſed fragments in concentric coats, ſeems eaſily explained; for I have already pointed out the facility with which the radii of the ſpheroids ſeparated at nearly the ſame diſtances from their centres, and the form of the fragments which reſulted, reſembling fragments of bombs.*

and theſe may have naturally formed the baſaltic columns.

If this idea be not conſidered as entirely diveſted of plauſibility, I may venture to extend the ſame principle, to account for the wonderful regularity of the priſmatic configuration of

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* Even granite has been frequently obſerved to affect globular decompoſition, and diviſion into fragments of concentric coats. This mode of decompoſition extends to many ſubſtances, that Werner has called the formation it ſeems to indicate, "*abgeſonderte ſtücke*," which has been rendered in Engliſh, *diſtinct concretions*.

baſaltic

Enumeration of facts and circumſtances.

1. Radiated ſpheroids formed near the ground,

in one layer.

baſaltic columns, and alſo for their articulations. If we ſuppoſe that a maſs of fluid baſalt has filled a valley to an indefinite depth and extent, the proceſs of arrangement in its particles muſt be induced by the removal of its heat or moiſture, according as its ſolution is igneous or aqueous. This can only be done by the action of the atmoſphere on its upper ſurface, and by the ground on which it repoſes abſorbing the heat or moiſture from its under ſurface. From the variations of the atmoſphere, its action muſt be irregular; and, from the perpetual change of the parts in contact with the heated or moiſt ſurface, its operations will always be nearly as active as at firſt, allowance being made for its variations. But the abſorption of the ground will be regular, and regularly diminiſhing in activity, in proportion as the parts near the maſs approach nearer to the ſame temperature, or ſame moiſture, with the maſs above; and its abſorptions can only be carried on by its tranſmiſſion of heat or moiſture to the ſolid rocks below. From theſe conſiderations it ſeems evident, that the arrangement of the part of the baſalt near the ground, will be begun with more energy than it can be continued, and that the reſults will be more ſlow and regular than the arrangement induced by the perpetual though variable action of the atmoſphere. After the firſt ſtage in the proceſs of arrangement has been performed, and a ſtratum, if I may ſo term it, of the jaſpedeous ſubſtance extended over the ſurface of the ground, there ſeems no reaſon to doubt that a number of radiated ſpheroids would be generated in it, which would probably have all their centres about the ſame diſtance from the ground; and, as the arranging power undergoes a gradual diminution of energy, it is not probable that two rows in height of them ſhould be formed at once, as that would indicate a haſty proceſs, which had prepared a greater maſs of matter for their almoſt ſimultaneous formation. From theſe conſiderations, there ſeems no improbability in ſuppoſing, that in the arrangement of a maſs of fluid baſalt, a ſingle layer of radiated ſpheroids would be formed, repoſing on the ground which ſupported the maſs.

2. Theſe would take hexagonal peripheries at the places of ſide contact;

I have already ſtated, that when the radii of two ſpheroids came into contact, no penetration enſued, but the two bodies became mutually compreſſed, and ſeparated by a plane, well defined, and inveſted with a ruſty colour. I alſo ſtated, that when ſeveral ſpheroids encountered, they formed one another into priſms with well defined angles. In a ſtratum compoſed of an indefinite number in ſuperficial extent, but only one in height, of impenetrable ſpheroids, with nearly equidiſtant centres, if their peripheries ſhould come in contact on the ſame plane, it ſeems obvious that their mutual action would form them into hexagons; and, if theſe were reſiſted below, and there was no oppoſing cauſe above them, it ſeems equally clear, that they would extend their dimenſions upwards, and thus form hexagonal priſms, whoſe length might be indefinitely greater than their diameters. The farther the extremities of the radii were removed from the centre, the nearer would be their approach to paralleliſm; and the ſtructure would be finally propagated by nearly parallel fibres, ſtill keeping within the limits of the hexagonal priſm with which their incipient formation commenced; and the priſms might thus ſhoot to an indefinite length into the undiſturbed central maſs of the fluid, till their ſtructure was deranged by the ſuperior influence of a counteracting cauſe, which would be provided by the action of the atmoſphere on the upper ſurface of the baſalt. If this arrangement exiſted, the ſame cauſe that determined the concentric fractures of the fibres of the ſperoids, would produce convex articulations in the lower joints of the priſms; and, in proportion as the centre from which they were generated became more remote, the articulations would approximate to planes. If the generating centres were not equidiſtant, the forms of the pillars would be irregular; and the irregularity would be in proportion to the diverſity of diſtance between the centres. If the difference was great, the number of ſides would be altered, and they might be found pentagonal, tetrahedral, and trihedral. As the compreſſion of the fibres would be greateſt in the level of the generating centres, the lower part of the priſms would be moſt compact.

and other ſpheroids formed above them would produce articulated columns.

The facts attending the production of baſaltic columns agree with the foregoing.

All theſe conditions ſeem to be fulfilled, in the actual conformation of baſaltic columns; for, in every inſtance I am acquainted with, they appear to have been formed in the tranquil boſom of the maſs, as they have been originally maſked by amorphous trap, and their priſmatic ſtructure is only diſplayed by the removal of this covering. This has been variouſly effected, ſometimes by the apparent diſrupture of rocks, ſometimes by the exterior portions of the maſs being

Baſaltic veins.

thrown down by the failure of the ground on which it ſtood, ſometimes by the violence of the waves, and not unfrequently by the working of quarries. In moſt inſtances, theſe operations have only removed the covering from one ſide of the colonnade; and it remains crowned, and generally ſurrounded, by an immenſe amorphous maſs. Where there are two ranges of columns, with an intervening amorphus ſtratum, it is probable that the upper is the reſult of a ſecond inundation of fluid baſalt. It is well known that baſaltic columns are moſt ſolid at the bottom; and their convex articulations have been repeatedly obſerved. Since theſe conſiderations occurred to me, I have had no opportunity of examining, whether the diviſions approach nearer to the plane ſurfaces as they recede from the centre from which the priſms was generated, nor whether below that centre the convex ſurface of the articulations is inverted; but I think it by no means improbable, that ſubſequent obſervations may eſtabliſh this to be the caſe, and thus confer on this hypotheſis nearly all the demonſtration of which it is ſuſceptible. I may however add, that the phenomena of priſmatic diviſion in baſaltic veins, perfectly coincide with what might be inferred from the data upon which my reaſoning has proceeded. In veins, it is obvious that the refrigerating or abſorbing cauſe muſt operate with nearly equal force on each ſide of the vein; and it follows, that two ſets of priſms would be generated, which would be horizontal inſtead of perpendicular, and that, unleſs a maſs of amorphous baſalt was interpoſed between them, they muſt form a diviſion in the middle of the vein, as, from the mutual impenetrability of their fibres, they could not incorporate. The coincidence of the exiſting phenomena with theſe concluſions, is ſufficiently remarkable; for, in numerous obſervations I have made on the baſaltic veins which effect the priſmatic configuration, I found the priſms were always horizontal, and often, that there were two ranges of them. One of their ends applied to the wall of the veins, the other frequently united to an amorphous maſs which ſeparated them; and, when no ſuch intermedium occurred, there was invariably a diviſion in the middle of the vein. Not unfrequently, the veins contain three ſets of priſms; a range of ſmall ones on each ſide, and of much larger ones in the middle. In this caſe, the little priſms are always ſeparated from the large ones, and the diviſions of the large ones are very irregular.[*]

Theſe deductions do not ſuppoſe either the aqueous or igneous formation of baſaltes to be eſtabliſhed.

After the ſtatement of my opinion, that perfect ſimilarity of ſtructure may exiſt in the products of aqueous and igneous formation, it will hardly be neceſſary to conclude theſe obſervations with remarking, that I ſhould not conſider the eſtabliſhment of theſe peculiar modes of arrangement as the ſlighteſt demonſtration of the igneous origin of baſalt. It appears to me, that the truth of my deductions is entirely independent of either theory, and that, if ever the period ſhould arrive when the origin of baſalt ſhall be determined by irrefragable demonſtration, the inferences I have drawn may be accommodated with equal facility to either mode of agency.[†]

The

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[*] The obſervations alluded to were made during the courſe of laſt ſummer (1803), on the very numerous baſalt veins, or, as they are there called, Whin Dykes, which traverſe the red ſandſtone and red ſandſtone breccia, which forms the greateſt part of the coaſt of the Firth of Clyde, between Greenock and the Largs.

[†] Mr. Keir, in his paper on the cryſtallizations formed in glaſs, ſuggeſts the probability of baſaltic pillars being formed by the cryſtallization of vitreous lavas. See Philoſophical Tranſactions for 1776, Vol. LXVI. page 530.

Dolomieu was of opinion, that the priſmatic form was peculiar to lavas which had flowed into the ſea; and he attributed it to the ſhrinking of the maſs: his deſcription of the appearances exhibited by what he calls the priſmatic lavas at the foot of Etna, merits quotation.

" In the lavas of Etna, the form and dimenſions of the columns vary as much as the manner in which they are grouped; hexaedral and pentaedral priſms are moſt abundant; then the tetraedral, the triedral, heptaedral, and octaedral. The leaſt I have ſeen are only four inches diameter; others are more than three feet; they are commonly of a ſingle ſhoot, which is ſometimes 60 feet high; others are divided by articulations, which are from one to ſix feet aſunder.

" I have more than once obſerved a large column divide into ſeveral ſmaller in its upper part. The columns are generally larger near the top than the bottom of the ſtream of lava, becauſe they ſubdivide; and they are always leaſt in that part of the ſtream of lava which firſt entered the water, the refrigeration being more prompt, Other rocks alſo affect the columnar form.

The immenſe magnitude of ſome baſaltic columns, the extreme regularity of their priſmatic configuration, and the peculiar ſtructure of their articulations, have directed the attention of naturaliſts to them, much more than to any of the other rocks which affect the columnar form. Yet many of theſe are ſufficiently remarkable to deſerve more particular notice than has generally been paid to them; and they afford moſt illuſtrative proof, that this configuration is not confined to either the aqueous or igneous formation; for ſome lavas, univerſally allowed to be ſuch, are priſmatic.* Columns of porphyry

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prompt, and its effects more marked. Sometimes the columns are placed perpendicularly ſide by ſide, and form vertical walls, which are ſometimes more than 100 feet high, and a league long; ſometimes they are heaped obliquely, horizontally, and in all poſitions. Some, without being divided in their length, are larger at one end than the other; and then they are arranged like wood piled up, with all the ſmall ends at one ſide; ſometimes they are formed into pyramidal bundles, by parting from a common center; and, finally, there are ſome which, by their reunion, form large balls. Theſe radii of lava, which are rather pyramidal than priſmatic, reſemble thoſe of the globular pyrites, ſtriated from the center to the circumference, which are found in the chalk of Champagne.

"On the ſhore of la Trezza, near the mole, there is a very curious group of little articulated priſms, which iſſue from a common center, and form faſciculi ſingularly twiſted. The articulations are marked, but the ſpecies of vertebræ do not ſeparate. In the heart of the mountain on which ſtands the Caſtello di Jaci, there are large balls, from two to four feet in diameter, reſembling in form the large pyrites in the chalk of Champagne. Theſe balls of lava are formed of pyramidal columns, united by their points in a common center." *Catalogue des Laves de l'Etna*, page 453.

The diviſion of the upper part of baſaltic columns into ſeveral ſmaller ones, has alſo been obſerved in the baſaltic columns of Fairhead, by Dr. Richardſon. See Nicholſon's Journal, 4to. Vol. V. page 321.

* Almoſt all the priſms at the foot of Etna, deſcribed by Dolomieu, are of dubious origin; moſt of them are probably baſalt. The columns of the Vincentine are of the ſame ſubſtance, and ſo are the priſmatic lavas of Auvergne, and of the Vivarais. The bed of lava at la Scala, near Portici, is divided by vertical fiſſures, which give it the aſpect of irregular columns. At Aquapendente, in a quarry of undoubted lava, near the road, are ſome much more

Inſtances: Porphyry, petroſilex, argillaceous ſchiſtus, rubble ſlate, limeſtone, &c.

porphyry are not rare; and, among other places, are found near Dreſden, ſeveral feet in length, and not more than two inches in diameter. Columns of petroſilex compoſe a large portion of a mountain near Coniſtone lake. Very perfect quadrangular priſms of argillaceous ſchiſtus are found near Llanwrſt. Rubble ſlate aſſumes the columnar form at Barmouth. The limeſtone near Cyfartha, in Glamorganſhire, is divided into very regular acute rhomboidal priſms: even the ſandſtone of the ſame diſtrict is not unfrequently columnar; and one of the beds of gypſum at Montmartre is diſtinctly divided into pretty regular columns. Sandſtone, clay, argillaceous iron ore, and many other ſubſtances, become priſmatic by torrefaction; and the priſms of ſtarch formed in drying have often been conſidered as illuſtrative of baſaltic formations.

Mere contraction may produce effects reſembling theſe, but leſs regular.

I am very far from conceiving, that all theſe configurations are influenced by ſuch ſyſtematic arrangements as have determined the form of ſome baſaltic columns. I conſider moſt of them as ſolely attributable to contraction; which is only a farther extenſion of the aggregative force, and muſt be regulated by the texture, the form, and the poſition of the maſs. Where the texture of the maſs is homogeneous, and its contractions uniform, its dimenſions may be diminiſhed, without its continuity being deſtroyed, provided its aggregation be ſo ſtrong as to overcome the *vis inertiæ* of the maſs, and its adheſion to other ſubſtances. But, when the reſiſtance is ſufficient to overcome the aggregation, the maſs will be rent by fiſſures perpendicular to the direction in which the greateſt reſiſtance to its contraction takes place, or, in other words, by fiſſures perpendicular to its greateſt ſurface: for it is from the extremities of the greateſt ſurface, that the largeſt quantity of

Developement of the effects of contraction in producing ſymmetric fractures.

perfect priſms; but the moſt beautiful I have ſeen, are the ſmall ones from Ponza. The columns at Bolſena are ſaid to be baſalt. Thoſe of the Euganean hills are very irregular in form; in their texture they are certainly wholly unlike granite, which Mr. Strange thought they reſembled. I believe them to be lava.

The mention of ſome columnar formations that follows, is by no means intended as an enumeration of them. I have confined myſelf to thoſe which I have either inſpected in their natural ſituation, or of which I have ſeen numerous ſpecimens.

matter muſt traverſe the greateſt ſpace, in order that the contraction may be performed without breach of continuity; therefore, if it be an extenſive tabular maſs, it will be divided into priſms, by fiſſures perpendicular to its ſurfaces. The power of aggregation would determine theſe priſms to be hexagonal, as that form contains the greateſt quantity of matter in the leaſt ſurface, of any priſms that can be united without interpoſing priſms of other forms. But this would require the texture, the contraction, the thickneſs of the maſs, and its adheſion to ſurrounding ſubſtances, to be every where preciſely the ſame; and, as theſe conditions can never be fulfilled in an extenſive formation, all the irregularities that are found muſt neceſſarily enſue. The ſame rule that determines the fiſſures of a tabular maſs to be perpendicular to its ſurfaces, muſt determine the rents in a ſpheroid to be directed from its periphery to its centre.

They do not account for the regular form of baſaltes.

Though theſe conſiderations may be ſufficient to explain the tendency to diviſion into priſms, which is ſo generally extended, and which has produced many of thoſe abortions that have been dignified with the name of columns, becauſe they have occurred in lavas and in rocks of trap formation, they are utterly inadequate to illuſtrating the formation of the more perfect baſaltic priſms: they offer no means of accounting for the extreme regularity of the ſides and the preciſion of the angles, for the articulations, for the cloſe contact in which the perfect columns are placed to one another, not for their mutual adheſion, which is ſo ſtrong, that it often requires conſiderable violence to ſeparate them. Theſe facts are in abſolute contradiction to all idea of retreat or contraction, and ſeem to me to coincide perfectly with the explanation of their origin which I have already preſumed to lay before you.

I have the honour to be, &c.

GREGORY WATT.

VI.

Improved Conſtruction of WOULFE's *Apparatus. By* W. B.

To Mr. NICHOLSON.

SIR,

Improvement in Woulfe's apparatus.

EVERY one who has been in the habit of uſing a Woulfe's apparatus, muſt have experienced the difficulty of fixing the tubes of communication, and the continual riſk of breaking them. To obviate this inconvenience, I would ſuggeſt the following mode of conſtructing the apparatus, which, if you think any improvement upon the preſent method, cannot be rendered more generally uſeful than by inſertion in your Journal.

I am, Sir,
Your very obedient humble ſervant,
W. B.

Feb. 5, 1805.

Fig. 1 repreſents one of the bottles. The bent tube, A B C, being attached to the outſide of the bottle when both heated, and entering it at C, may be made as ſtrong as is thought conſiſtent with neatneſs.

The part H B of the tube is of a conical form, ſmall enough at A to be inſerted in a perforated cork.

The ſhort tube D E, on the oppoſite ſide, is alſo of a conical form, large enough at E to admit a perforated cork.

The opening at F being intended for the tube of ſafety.

Fig. 2 repreſents the apparatus put together, the receiver being added as of a convenient form for connecting with the other veſſels.

The ſmall tube A B of one bottle, is inſerted in the perforated cork fitted to the tube D E of the preceding one, and the joinings ſecured in the uſual way by luting.

The tubes of ſafety being firſt ſecurely fixed, and the cork cloſely fitted in the tube D E, any number of bottles may be ſucceſſively added, without the leaſt riſk of deranging thoſe that are already connected.

Obſervation

VII.

Obſervations on the Change of ſome of the proximate Principles of Vegetables into Bitumen; with Analytical Experiments on a peculiar Subſtance which is found with the Bovey Coal. By CHARLES HATCHETT, *Eſq. F. R. S. From the Philoſophical Tranſactions for* 1804.

§ I.

Converſion of organized bodies into foſſils.

ONE of the moſt inſtructive and important parts of geology, is the ſtudy of the ſpontaneous alterations by which bodies formerly appertaining to the organized kingdoms of nature have, after the loſs of the vital principle, become gradually converted into foſſil ſubſtances.

The degrees are various.

In ſome caſes, this converſion has been ſo complete, as to deſtroy all traces of previous organic arrangement; but, in others, the original texture and form have been more or leſs preſerved, although the ſubſtances retaining this texture, and exhibiting theſe forms, are often decidedly of a mineral nature. Some, however, of theſe extraneous foſſils (as they are called) retain part of their original ſubſtance or principles, whilſt others can only be regarded as caſts or impreſſions.

Inſtances in the animal kingdom,

From the animal kingdom we may ſelect, as examples, the foſſil ivory, which retains its cartilage*; the bones in Gibraltar rock, conſiſting of little more than the earthy part or phoſphate of lime; the ſhells forming the lumachella of Bleyberg, which ſtill poſſeſs the luſtre and irideſcence of their original nacre; and the ſhells found at Hordwell in Hampſhire, and in Picardy, which are chiefly porcellaneous, but more or leſs calcined; alſo the foſſil echini and others, ſo commonly found in the limeſtone, chalk, and calcareous grit of this iſland, which, although they retain their original figure, are intirely, or at leaſt externally, formed of calcareous ſpar, incruſting a nucleus of flint or chalcedony. And if, in addition to theſe, we may be allowed to regard the more recent limeſtone and chalk ſtrata as having been principally or partly formed from the detritus of animal exuviæ, we ſhall poſſeſs a complete ſeries of gradations, commencing with animal ſubſtances analogous in properties to thoſe which are recent, and terminating in bodies decidedly mineral, in which all veſtiges of organization have been completely deſtroyed.

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* I have alſo found the cartilage perfect, in the teeth of the mammoth.

and in the vegetable.

The vegetable kingdom has likewiſe produced many inſtances not leſs remarkable; and it is worthy of notice, that animal petrifactions are commonly of a calcareous nature, while, on the contrary, the vegetable petrifactions are generally ſiliceous *.

It is not, however, my intention here to enter into a minute diſcuſſion concerning the formation of theſe extraneous foſſils; I ſhall therefore proceed to conſider other equally or perhaps more important changes, which organized bodies, eſpecially vegetables, appear to have ſuffered, (after the extinction of the principle of life,) by being long buried in earthy ſtrata, and by being thus expoſed to the effects of mineral agents.

§ II.

Bituminous matters are derived from organized matters; chiefly vegetables.

The principal object I have in view, is to adduce ſome additional proofs, that the bituminous ſubſtances are derived from the organized kingdoms of nature, and eſpecially from vegetable bodies; for, although many circumſtances ſeem to lead to the opinion, that the animal kingdom has in ſome meaſure contributed to the partial formation of bitumen, yet the proofs are by no means ſo numerous, nor ſo poſitive, as thoſe which indicate the vegetable kingdom to have been the grand ſource from which the bitumens have been derived. But this opinion, (founded upon very ſtrong preſumptive evidence,) although generally adopted, is however queſtioned by ſome perſons; and I ſhall therefore bring forward a few additional facts, which will, I flatter myſelf, contribute to demonſtrate, that bitumen has been, and is actually and immediately formed, from the reſin, and perhaps from ſome of the other juices of vegetables.

Pure bitumens differ much from vegetable juices;

The chemical characters of the pure or unmixed bitumens, ſuch as naptha, petroleum, mineral tar, and aſphaltum, are, in certain reſpects, ſo different from thoſe of the reſins and other inſpiſſated juices of recent vegetables, that, had the former never occurred but in a ſeparate and unmixed ſtate, no poſitive inference could have been drawn from their pro-

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* Pyrites, ochraceous iron ore, and fahlertz, are alſo occaſionally found in the forms of vegetable bodies.

perties in proof of their vegetable origin. Fortunately, however, they have been more frequently found under circumſtances which have ſtrongly indicated the ſource from whence they have been derived; and much information has been acquired from obſervations made on the varieties of turf, bituminous wood, and pit coal, on the nature of their ſurrounding ſtrata, on the veſtiges of animal and vegetable bodies which accompany them, and on various other local facts; all of which tend conſiderably to elucidate the hiſtory of their formation, and to throw light upon this intereſting part of geology.

but the progreſſive changes are inſtructive in theſe reſpects.

Some inſtances have already been mentioned, which ſhow that foſſil animal ſubſtances form a ſeries, commencing with ſuch as are ſcarcely different from thoſe which are recent, and terminating in productions which have totally loſt all traces of organization.

Similar inſtances are afforded by the vegetable kingdom; but without entering into a minute detail of every gradation, I ſhall only cite three examples in this iſland, namely,

Series of change. 1. Submarine wood.

1. The ſubmarine foreſt at Sutton, on the coaſt of Lincolnſhire, the timber of which has not ſuffered any very apparent change in its vegetable characters *.

2. Bovey coal in all its gradations.

2. The ſtrata of bituminous wood (called Bovey Coal) found at Bovey, in Devon; which exhibits a ſeries of gradations, from the moſt perfect ligenious texture, to a ſubſtance nearly approaching the characters of pit coal, and, on that account, diſtinguiſhed by the name of Stone Coal.

3. Perfect pit-coal.

3. And laſtly, the varieties of pit coal, ſo abundant in many parts of this country, in which almoſt every appearance of vegetable origin has been deſtroyed.

The ſecond ſtate offers the moſt inſtructive ſubject for reſearch.

The three examples above-mentioned, appear to form the extremities and centre of the ſeries; but as, from ſome local circumſtances, the proceſs of carbonization, and formation of bitumen, has not taken place in the firſt inſtance, and as theſe effects have proceeded to the ultimate degree in the laſt, it ſeems moſt proper that we ſhould ſeek for information, and for poſitive evidence, in the ſecond example, which appears to be the mean point, exhibiting effects of natural operations, by which bitumen and coal have been imperfectly and partially

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* Account of a ſubmarine Foreſt on the Eaſt Coaſt of England, by Dr. Correa de Serra. Phil. Tranſ. for 1799, p. 145.

formed, without the abſolute obliteration of the original vegetable characters; and, although I have ſelected the Bovey coal as an example, becauſe it is found in this country, we muſt recollect that ſimilar ſubſtances, or ſtrata of bituminous wood, are found in many parts of our globe; ſo that the example which has been more immediately choſen, is neither rare nor partial *.

The nature, however, of the various kinds of bituminous wood, may in ſome reſpects be different; but this I have not as yet had the means of aſcertaining; I ſhall therefore only ſtate the facts reſulting from experiments made on Bovey coal, and more eſpecially on a peculiar bituminous ſubſtance with which it is accompanied. But, before I enter into theſe particulars, it will be proper to mention a very remarkable ſchiſtus, with which I was, ſome months ſince, favoured by the Right Hon. Sir Joſeph Banks.

Remarkable ſchiſtus,

§ III.

from Iceland,

This ſchiſtus was found by Sir Joſeph, in the courſe of his tour through Iceland, near Reykum, one of the great ſpouting hot ſprings, diſtant about twenty-four Engliſh miles from Hafnifiord; but circumſtances did not permit him to aſcertain the extent of the ſtratum.

conſiſting very much of alder leaves.

The ſingularity of this ſubſtance is, that a great part of it conſiſts of leaves, which are evidently thoſe of the alder, interpoſed between the different lamellæ. I do not mean mere impreſſions of leaves, ſuch as are frequently found in many of the ſlates, but the real ſubſtance, in an apparently half charred ſtate, retaining diſtinctly the form of the leaves, and the arrangement of the fibres.

External characters.

The ſchiſtus is light, brittle, of eaſy oxfoliation, in the tranſverſe fracture earthy, and of a pale brown colour; but, when longitudinally divided, the whole ſurface conſtantly preſents a ſeries of the leaves which have been mentioned, uniformly ſpread, and commonly of a light gray on the upper ſurface, and of a dark brown on the other; the fibres on the

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* Strata of bituminous wood are found in various parts of France, in the vicinity of Cologne, in Heſſe, Bohemia, Saxony, Italy, and eſpecially in Iceland, where it is known under the name of Surturbrand.

light gray ſurface being generally of a blackiſh-brown, which is alſo the colour aſſumed by the ſchiſtus when reduced to powder.

The leaves appeared to be in the ſtate of charcoal, by being extremely brittle, by the blackiſh brown colour, by deflagrating with nitre, by the manner of burning, and by forming carbonic acid. I was, however, ſoon convinced that the ſubſtance of theſe leaves was not complete charcoal, but might more properly be regarded as vegetable matter in an incipient ſtate of carbonization, which, although poſſeſſed of many of the apparent porperties of charcoal, ſtill retained a ſmall portion of ſome of the other principles of the original vegetable.

The leaves almoſt in the ſtate of coal;

My ſuſpicion was excited, partly by the odour produced during combuſtion, which rather more reſembled that of wood than that of charcoal, and partly by the brown ſolution formed by digeſting the powder of the unburned ſchiſtus in boiling diſtilled water; for, by various teſts I aſcertained, that the ſubſtance thus diſſolved was not of a mineral nature. In order, however, fully to ſatisfy myſelf in this reſpect, I digeſted 250 grains of the pulverized ſchiſtus with ſix ounces of water.

but burning with the odour of wood.

Digeſtion of the ſchiſtus in water, and examination of the ſolution.

The liquor was, as before, of a dark brown colour.

It had but little flavour.

Pruſſiate of potaſh, muriate of barytes, and ſolution of iſinglaſs, did not produce any effect; nitrate of ſilver formed a very faint cloud; ſulphate of iron was ſlowly precipitated, of a dark browniſh colour; and muriate of tin produced a white precipitate.

A portion of the ſolution, by long expoſure to the air, was partially decompoſed; and a quantity of a brown ſubſtance was depoſited, which could not again be diſſolved in water.

Another portion was alſo evaporated to dryneſs, and afforded a ſimilar brown ſubſtance, which was only partially ſoluble in water; and the reſiduum, in both of the above caſes, was found to be inſoluble in alcohol, and in ether.

When burned, it emitted ſmoke, with the odour of vegetable matter.

250 grains of the ſchiſtus, afforded about three grains of the above ſubſtance; and, when the properties of the aqueous ſolution are conſidered, ſuch as its partial decompoſition, and the depoſit which it yielded by expoſure to air, and by evaporation;

tion; the inſolubility of this depoſit when again digeſted with water, alcohol, or ether; the ſmoke and odour which it yielded when burned; and the precipitates formed by the addition of ſulphate of iron and muriate of tin to its ſolution; when theſe properties, I ſay, are conſidered, there ſeems much reaſon to conclude, that the ſubſtance diſſolved by water was vegetable extract, which had apparently ſuffered ſome degree of modification, but not ſufficient to annul the more prominent characteriſtic properties of that ſubſtance.

The powder of the ſchiſtus, which had been employed in the preceding experiment, was afterwards digeſted in alcohol during two days; and a pale yellow tincture was thus formed, which, by evaporation, left about one grain of a yellow tranſparent ſubſtance, poſſeſſing the properties of reſin.

It contained vegetable extract, and a little reſin,

It appears, therefore, that a ſubſtance very analogous to vegetable extract, and a ſmall portion of reſin, remain inherent in the leaves of this remarkable ſchiſtus.

but no tannin.

As ſolution of iſinglaſs did not produce any effect, there was reaſon to conclude, that the aqueous ſolution above-mentioned did not contain any tannin; but, as the tannin might be combined with the alumina of the ſchiſtus, I digeſted a portion of it in muriatic acid, which, after filtration, was evaporated almoſt to dryneſs, leaving, however, the acid in a ſlight exceſs. This was diluted with water; and afforded a blue precipitate with pruſſiate of potaſh, a yellowiſh precipitate with ammonia, and a white precipitate with muriate of tin, but not any with ſolution of iſinglaſs. The tannin which might have been contained in the recent vegetable, appears therefore to have been diſſipated or decompoſed, with the greater part of the other vegetable principles, excepting the woody fibre reduced to the ſtate of an imperfect coal, and the ſmall portions of extract and reſin which have been mentioned.

Previous to having made the analyſis, I had an idea, that this ſchiſtus might be a lamellated incruſtation, formed by the tufa of the hot ſprings; but, according to Mr. Klaproth's analyſis *, the tufa of Geyſer is compoſed of,

Silica - -	98
Alumina - - -	1.50
Iron - -	50
	100.

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* *Beiträge*; *Zweiter band*, p. 109.

It is therefore very different from the ſchiſtus, the component ingredients of which were aſcertained from the following analyſis.

ANALYSIS OF THE SCHISTUS FROM ICELAND*.

Analyſis of the ſchiſtus.

A. 250 grains, by diſtillation, yielded water, which, in the latter part of the proceſs, became ſlightly acid and turbid, =42.50 grains.

B. The heat was gradually increaſed, until the bulb of the retort was completely red hot. During the increaſe of the heat, a thick brown oily bitumen came over, which weighed 7.50 grains; it was attended with a copious production of hydrogen, carbonated hydrogen, and carbonic acid, the whole of which may be eſtimated at 23.75 grains.

C. The reſiduum was black, like charcoal, and weighed 176.25 grains; but, being expoſed to a ſtrong red heat in a crucible of platina, it burned with a faint lambent flame, and was at length reduced to a pale brown earthy powder, which weighed 122 grains; ſo that 54.25 grains were conſumed.

D. The 122 grains were mixed with 240 of pure potaſh; and, as ſome particles of charcoal remained, 50 grains of nitre were added, and the whole was ſtrongly heated, during half an hour, in a ſilver crucible. The maſs was then diſſolved in diſtilled water, and, muriatic acid being added to exceſs, the liquor was evaporated to dryneſs, and was again digeſted with muriatic acid much diluted; a quantity of pure ſilica then remained, which, after having been expoſed to a red heat, weighed 98 grains.

E. The liquor from which the ſilica had been ſeparated, was evaporated nearly to dryneſs, and added to boiling lixivium of potaſh; after the boiling had been continued for about one hour, the liquor was filtrated, and a quantity of oxide of iron was collected, which amounted to 6 grains.

F. Solution of muriate of ammonia was added to the preceding filtrated liquor; and, the whole being then heated, a copious precipitate of alumina was obtained, which, after having been made red-hot, weighed 15 grains.

Carbonate of ſoda cauſed the preceding liquor (after the ſeparation of alumina) to become ſlightly turbid, but not any precipitate could be collected.

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* The remaining ſpecimens are now in the Britiſh Muſeum, and in the collection of the Right Hon. Charles Greville.

By

Component parts.

By this analyſis, 250 grains of the ſchiſtus afforded,

		Grains.
Water - - -	A. -	42.50
Thick brown oily bitumen	B. -	7.50
Mixed gas (by computation)		23.75
Charcoal (by computation)	C. -	54.25
Silica - - - -	D. -	98
Oxide of iron - -	E. -	6
Alumina - - - -	F. -	15
		247.

But the water and vegetable matter muſt be regarded as extraneous; and, if they are deducted, the real compoſition of the ſchiſtus is nearly as follows.

Its component parts, excluſive of water and vegetable matter.

Silica - - - -	82.30
Alumina - - - -	12.61
Oxide of Iron - - -	5.
	99.91.

It evidently, therefore, belongs to the family of argillaceous ſchiſtus, although the proportion of ſilica is more conſiderable than has been found in thoſe hitherto ſubjected to chemical analyſis.

The ſchiſtus has not been noticed by von Troil, nor by any of thoſe who have written concerning Iceland; for the ſlate which was ſent to Profeſſor Bergmann by the former, and which is mentioned by the latter in one of his letters, is there expreſsly ſtated to be the common aluminous ſlate containing impreſſions *.

§ IV.

Queſtion, whether the circumſtances of the production of this ſchiſtus were peculiar.

From the experiments which have been related, we find that the leaves contained in the Iceland ſchiſtus, although they are

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* Letters on Iceland, by Uno von Troil, p. 355.

Mr. Faujas St. Fond has however deſcribed a ſchiſtus nearly ſimilar, which is found near Roche-Seauve, in the Vivarais. The ſtratum extends about two leagues; and the only difference is, that, according to Mr. St. Fond, the ſchiſtus at Roche-Seauve is of the nature of marble, or, as he terms it, argillo-calcareous, whereas this of Iceland is ubdoubtedly argillaceous. From Mr. St. Fond's account, it does not appear that the vegetable leaves contained in the ſchiſtus of Roche-Seauve have been chemically examined. *Eſſai de Geologie*, par. B. Faujas St. Fond, Tome I. pp. 128 and 134.

apparently reduced almoſt to the ſtate of charcoal, nevertheleſs retain ſome part of their original proximate principles, namely, extract and reſin. This, of itſelf, is undoubtedly a remarkable fact; but, if it were unſupported by any other, the only inference would be, that the ſchiſtus was moſt probably of very recent formation, and had been produced under peculiar circumſtances.

requires an attention to other mineral bitumens.

I was deſirous, therefore, to diſcover ſome ſimilar caſes, which might ſerve as additional corroborative proofs of the gradual alterations by which vegetable bodies become changed, ſo as at length to be regarded as forming part of the mineral kingdom; and, from the reaſons which have been ſtated in the commencement of this paper, as well as from a certain ſimilarity in the external characters of the ſubſtance compoſing the leaves above-mentioned with thoſe of the Bovey coal, I was induced to make this laſt alſo a ſubject of chemical inquiry.

Bovey coal.

In the Philoſophical Tranſactions for the year 1760 [*], ſome remarks on the Bovey coal, and an account of the ſtrata, are ſtated, in a letter from the Rev. Dr. Milles to the Earl of Macclesfield. The object, indeed, of the author, was to eſtabliſh that this and ſimilar ſubſtances are not of vegetable, but of mineral origin; and, to prove this, he adduces a great number of caſes, moſt of which, however, in the preſent ſtate of natural hiſtory and of chemiſtry, muſt be regarded as proving the contrary; whilſt others, mentioned by him, ſuch as the Kimmeridge or Kimendge coal, are nothing more than bituminous ſlates, and of courſe are of a very different nature.

Its hiſtory.

Dr. Milles's account of the varieties of the Bovey coal, and of the ſtate of the pits at that time, appears to be very accurate; and, for the preſent ſtate, or at leaſt ſuch as it was in 1796, I ſhall beg leave to refer to a paper of mine, publiſhed in the fourth volume of the Tranſactions of the Linnean Society [†]; for, as this is more immediately a chemical inveſtigation, I wiſh to avoid, as much as poſſible, entering into any minute detail of geological circumſtances.

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[*] Vol. LI. p. 534.

[†] Obſervations on bituminous Subſtances, p. 138. See alſo Parkinſon's Organic Remains of a former World. Vol. I. p. 126.

Its ſtrata agree with thoſe of ſurturbrand.

It may however be proper to obſerve, that the Bovey coal is found in ſtrata, correſponding in almoſt every particular with thoſe of the ſurturbrand in Iceland, deſcribed by Von Troil*, and by Profeſſor Bergmann †. The different ſtrata of both theſe ſubſtances are likewiſe ſimilar, being compoſed of wood or trunks of trees, which have completely loſt their cylindrical form, and are perfectly flattened, as if they had been ſubjected to an immenſe degree of preſſure ‡.

The

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* Von Troil's Letters, p. 42.

† *Opuſcula Bergmanni*, Tom. III. *De Productis Volcaniis*, p. 239.

‡ Bergmann, in the diſſertation above quoted, accurately deſcribes this appearance of the ſurturbrand, and then ſays, "Quæ autem immanis requiritur vis, ut truncus cylindricus ita complanetur? *Nonne antea particularum nexus putredinis quodam gradu fuerit relaxatus?* Certe, niſi compages quodammodo mutatur, quodlibet pondus incumbens huic effectui erit impar. Ceterùm idem obſervatur phænomenon *in omni ſchiſto argillaceo.*" This is certainly a very curious fact; and the learned Profeſſor, with his uſual acuteneſs, rejects the idea that mere weight can have been the cauſe. As a farther proof alſo, he afterwards obſerves, "Orthoceratitæ, quæ *in ſtrato calcareo conicam figuram perfectè ſervant*, in ſchiſto *planum fere triangulare compreſſione efficiunt.* Idem valet de piſcibus, conchis, inſectiſque petrefactis." And again, "Obſervatu quoque dignum eſt, quod *idem reperiatur effectus, quamvis ſtratum calcareum ſub ſchiſto collocatum ſit, et majori ideo pondere comprimente onuſtum.*" *De Productis Volcaniis*, p. 240. It is evident, therefore, that weight alone has not produced this effect; and Bergmann's idea, that the ſolidity of the vegetable bodies may have undergone ſome previous change, in the manner of incipient putrefaction, by moiſture, and by becoming heated in the maſs, muſt be allowed to be very probable. But bodies ſuch as ſhells could not be thus affected; and therefore they muſt have been expoſed to ſome mechanical effect, peculiar to argillaceous ſtrata; which effect, however, from the circumſtances which have been adduced, evidently could not have reſulted from the mere preſſure of the ſuperincumbent ſtrata. To me, therefore, it ſeems not very improbable, that, together with a certain change in the ſolidity of vegetable bodies, produced in the manner imagined by Bergmann, and, together with ſome degree of ſuperincumbent preſſure, a real and powerful mechanical action has been exerted, by the contraction of the argillaceous ſtrata, in conſequence

of

External characters of Bovey coal.

The Bovey coal is commonly of a chocolate-brown, and ſometimes almoſt black. The quality and texture of it are various in different ſtrata; from ſome of theſe, it is obtained in the form of ſtraight flat-pieces, three or four feet in length, reſembling boards, and is therefore called Board Coal. Others have an oblique, wavy, and undulating texture, and, as Dr. Milles obſerves, have a ſtrong reſemblance to the roots of trees, from which, moſt probably, this ſort has in a great meaſure been formed.

Some kinds alſo appear to be more or leſs intermixed with earth; but that which produces the moſt powerful and laſting fire, is called ſtone coal; it is black, with a gloſſy fracture; has little or none of the vegetable texture; is more ſolid and compact than the others, being almoſt as heavy as ſome of the pit coals, the nature of which it ſeems very nearly to approach.

For chemical examination, I ſelected ſome of the coal which had a wavy texture, and rather a gloſſy fracture; the quality of this ſort being apparently intermediate between the others, as it retains completely the marks of its vegetable origin, while, at the ſame time, it poſſeſſes every perfect character of this ſpecies of coal.

Chemical examination of Bovey coal.

A. 200 grains of the Bovey coal, by diſtillation, yielded,

		Grains.
1. Water, which ſoon came over acid, and afterwards turbid, by the mixture of ſome bitumen - -		60
2. Thick brown oily bitumen - - - -		21
3. Charcoal - - - - - - - -		90
4. Mixed gas, conſiſting of hydrogen, carbonated hydrogen, and carbonic acid,	eſtimated at	29
		200.

The charcoal, in appearance, perfectly reſembled that which is made from recent vegetables. By incineration, about 4 grains of yellowiſh aſhes were left, which conſiſted of alumina, iron, and ſilica, derived moſt probably from ſome ſmall portion of the clay ſtrata which accompany the Bovey coal.

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of deſiccation; this, I believe, has not hitherto been much conſidered, but I am inclined, from many circumſtances, to attribute to it a very great degree of power.

But it is very remarkable, that neither the aſhes obtained from the charcoal of the Bovey coal, nor thoſe obtained from the leaves of the Iceland ſchiſtus, afforded the ſmalleſt trace of alkali.*

B. 200 grains of the Bovey coal, reduced to powder, were digeſted in boiling diſtilled water, which was afterwards filtrated, and examined; but I could not diſcover any ſigns of extract, or of any other ſubſtance.

C. 200 grains were next digeſted with ſix ounces of alcohol, in a very low degree of heat, during five days. A yellowiſh-brown tincture was thus formed, which, by evaporation, afforded a deep brown ſubſtance, poſſeſſing all the properties of reſin, being inſoluble in water, but ſoluble in alcohol, and in ether; it alſo ſpeedily melted, when placed on a red-hot iron, burned with much flame, and emitted a fragant odour, totally unlike the very unpleaſant ſmell produced by burning the coal itſelf, or by burning any of the common bituminous ſubſtances. The quantity, however, which could be extracted from 200 grains of the coal, by alcohol, was but ſmall, as it did not exceed 3 grains. But this ſmall quantity was ſufficient to prove, that although the Bovey coal does not contain any vegetable extract, like the ſchiſtus formerly mentioned, yet the whole of the proximate principles of the original vegetable have not been entirely changed; as a ſmall portion of true reſin, not converted into bitumen, ſtill remains inherent in the coal, although the bituminous part is much the moſt prevalent, and cauſes the fetid odour which attends the combuſtion of this ſubſtance.

It reſembles the ſchiſtus in its products as well as character; but it afforded no extract, but only reſin;

Upon a compariſon of the general external characters of the Bovey coal, with thoſe of the ſubſtance which forms the leaves contained in the Iceland ſchiſtus, a very great reſemblance will be obſerved; and this is farther confirmed, by the ſimilarity of the products obtained from each of them in the preceding experiments, with the ſingle exception, that the leaves contain ſome vegetable extract, which I could not diſcover in the Bovey coal. They agree however in every other reſpect; as they both conſiſt of woody fibre in a ſtate of ſemicarbonization, impregnated with bitumen, and a ſmall portion of reſin, perfectly ſimilar to that which is contained in many recent vegetable bodies; and thus it ſeems, that as the woody fibre, in theſe caſes, ſtill retains ſome part of its vegetable characters, and is but partially and imperfectly converted into coal, ſo, in like manner, ſome of the other vegetable principles have only ſuffered a partial change. Undoubtedly, there is every reaſon to believe that, next to the woody fibre, reſin is the ſubſtance which in vegetables paſſing to the foſſil ſtate, moſt powerfully reſiſts any alteration; and that, when this is at length effected, it is more immediately the ſubſtance from which bitumen is produced. The inſtances which have been mentioned corroborate this opinion; for the vegetable extract in one of them, and more eſpecially the reſin which was diſcovered in both, muſt be regarded as part of thoſe principles of the original vegetables which have remained, after ſome other portions of the ſame have been modified into bitumen.

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* This, as far as relates to the Bovey coal, has been alſo noticed by Dr. Milles, Phil. Tranſ. Vol. LI. p. 553. But wood, however long ſubmerged, is not deprived of alkali, unleſs it has more or leſs been converted into coal; for I have, ſince the reading of this paper, made ſome experiments on the wood of the ſubmerged foreſt at Sutton, on the coaſt of Lincolnſhire, and have found it to contain potaſh.

which moſt probably is that which affords the bitumen.

The ſmallneſs of the quantity of reſin obtained in both the preceding caſes, by no means invalidates the proof of the above opinion; but, as an additional confirmation of it, I ſhall now give an account of a very ſingular ſubſtance, which is found with the Bovey coal.

§ V.

Singular ſubſtance found with the Bovey coal.

Dr. Milles, in his remarks on the Bovey coal, (which I have ſeveral times had occaſion to notice in the courſe of this Paper,) ſtates, that "amongſt the clay, but adhering to the coal, are found lumps of a bright yellow *loam*, extremely light, and ſo ſaturated with petroleum, that they burn like ſealing wax, emitting a very agreeable and aromatic ſcent."*

It is a peculiar bitumen.

This ſubſtance, I alſo obſerved, when I viſited the Bovey coal-pits, in 1794 and 1796. At that time, however, it was ſcarce, and I could only procure one ſmall ſpecimen, which is now in the Britiſh Muſeum; but, from a curſory examination of it, I was convinced that it was a peculiar bituminous ſubſtance, and not loam impregnated with petroleum, as Dr. Milles had ſuppoſed. I could not then conveniently make a regular analyſis of it, and therefore contented myſelf with briefly deſcribing it, in a note annexed to my Paper on bituminous Subſtances.*

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* Phil. Tranſ. Vol. LI. p. 536.

Lately, however, my friend John Sheldon, Eſq. of Exeter, F. R. S. obligingly ſent me ſeveral pieces of it, together with ſpecimens of the different kinds of Bovey coal which have been mentioned; and thus I was enabled fully to aſcertain its real nature and properties.

DESCRIPTION OF THE BITUMEN FROM BOVEY.

Deſcription of the bitumen from Bovey.

It accompanies the Bovey coal, in the manner already deſcribed, and is found in maſſes of a moderate ſize.

The colour is pale browniſh ochraceous yellow.

The fracture is imperfectly conchoidal.

It appears earthly externally, but, when broken, exhibits a ſlight degree of vitreous luſtre.

The fragments are irregularly angular, and completely opaque at the edges.

It is extremely brittle.

It does not apparently become ſoftened, when held for ſome time in the hand, but emits a faint reſinous odour.

The ſpecific gravity, at temperature 65° of Fahrenheit, is, 1,135.

Some ſpecimens have dark ſpots, ſlightly approaching in colour and luſtre to aſphaltum; and ſmall portions of the Bovey coal are commonly interſperſed in the larger maſſes of this bitumen.

When placed on a heated iron, it immediately melts, ſmokes much, burns with a bright flame, and yields a very fragrant odour, like ſome of the ſweet-ſcented reſins, but which at laſt becomes ſlightly tainted with that of aſphaltum.

The melted maſs, when cold, is black, very brittle, and breaks with a gloſſy fracture.

EXPERIMENTS.

Experiments on the ſame.

A. 100 grains of this bitumen, when diſtilled until the bulb of the retort became red-hot, afforded,

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* Tranſactions of the Linnean Society, Vol. IV. p. 139.

1. Water

	Grains.
1. Water ſlightly acid - - - - - -	3
2. Thick brown oily bitumen, very ſimilar to that which was obtained from the Bovey coal, but poſſeſſing ſlightly the odour of vegetable tar - - -	45
3. Light ſpongy coal - - - - - -	23
4. Mixed gas, compoſed of hydrogen, carbonated hydrogen, and carbonic acid, (by computation,) -	29.

The coal yielded about three grains and a half of aſhes, which conſiſted of alumina, iron, and ſilica, with a trace of lime.

B. The bitumen was not affected by being long digeſted in boiling diſtilled water.

C. By digeſting 100 grains in lixivium of pure potaſh, a brown ſolution was formed; this was ſaturated with muriatic acid, and a brown reſinous precipitate was obtained, which weighed 21 grains.

D. A portion was digeſted in nitric acid: at firſt, much nitrous gas was evolved, and, after the digeſtion had been continued for nearly 48 hours, a part was diſſolved, and formed an orange-coloured ſolution, which did not yield any precipitate, when ſaturated by the alkalis, or by lime; the colour only became more deep, and, by evaporation, a yellow viſcid ſubſtance was obtained, which was ſoluble in water. The above nitric ſolution poſſeſſed every property of thoſe nitric ſolutions of reſinous ſubſtances which I have mentioned in a former paper.*

E. The benzoic and ſuccinic acids were not obtained from this ſubſtance, by any of the methods uſually employed.

F. Alcohol almoſt immediately began to act upon this bitumen; and, being added at different times, gradually diſſolved a conſiderable part of it. The ſolution was reddiſh-brown, and has a reſinous odour; by the addition of water it became milky, and, by evaporation, afforded a dark brown ſubſtance, which had every property of reſin, whilſt the reſiduum left by the alcohol poſſeſſed thoſe properties which characterize aſphaltum.

The following analyſis was then made, to diſcover the proportions of the component ingredients.

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* Phil. Tranſ. for 1804, p. 198.

ANALYSIS OF THE BITUMEN FROM BOVEY.

Analyſis of the bitumen from Bovey.

A. 100 grains, reduced to a fine powder, were digeſted during 48 hours with ſix ounces of alcohol, the veſſel being placed in ſand moderately warmed. A deep reddiſh-brown tincture was thus obtained; and the operation was again twice repeated, with other portions of the ſame menſtruum, until it ceaſed to act upon the reſiduum.

The whole of the ſpirituous ſolution (which had been cautiouſly decanted) was then ſubjected to a very gradual diſtillation in an alembic, and yielded a brown fragant reſin, which weighed 55 grains.

B. The reſiduum, which could not be diſſolved by alcohol, was digeſted in boiling diſtilled water, but this did not act upon it; the whole was therefore collected on a filter, was gradually dried, without heat, by mere expoſure to the air, and then weighed 44 grains.

Theſe 44 grains conſiſted of a light, porous, pale-brown ſubſtance, which, being melted, formed a black, ſhining, brittle maſs. It burned with the odour of aſphaltum, but rather leſs diſagreeable, owing moſt probably to a ſmall portion of the reſin, which had not been completely extracted by the alcohol. It was inſoluble in water, and in alcohol, but was readily diſſolved by heated fat oils; and in every other particular was found to poſſeſs the properties of aſphaltum.

The 44 grains of aſphaltum, when burned, left a reſiduum, which weighed three grains, and conſiſted of alumina, ſilica, and iron.

By this analyſis it appears, that the bitumen which accompanies the Bovey coal, is a peculiar and hitherto unknown ſubſtance, which is partly in the ſtate of vegetable reſin, and partly in that of the bitumen called aſphaltum, the reſin being in the largeſt proportion, as 100 grains of the above-mentioned ſubſtance afforded,

Its component parts.

Reſin - - - - - -	55
Aſphaltum - - - - -	41
Earthy reſiduum - - -	3
	99.

Thus we have an inſtance of a ſubſtance being found under circumſtances which conſtitute a foſſil, although the characters of it appertain partly to the vegetable, and partly to the mineral kingdom.

§ VI.

§ VI.

Experiments on bitumens by the action of alcohol.

The powerful action which alcohol exerts on moſt of the reſins, may juſtly be regarded as forming a marked diſtinction between thoſe ſubſtances and the bitumens. But, as ſome of the bitumens are acted upon by alcohol, in a ſlight degree, I was deſirous to aſcertain whether a ſmall portion of reſin was contained in any of theſe; or, if that was not the caſe, I wiſhed to determine the nature of the ſubſtances which could be ſeparated, although very ſparingly, by this menſtruum. I therefore made the following comparative experiments, on the ſoft brown elaſtic bitumen from Derbyſhire; on the genuine aſphaltum; on very pure cannel coal; and on the common pit coal.

100 grains of each were digeſted with three ounces of alcohol, in matraſſes placed in warm ſand, during five days, ſome alcohol being occaſionally added, to ſupply the loſs cauſed by evaporation. After the above-mentioned period had elapſed, the liquid contained in each matraſs was poured into ſeparate veſſels.

Reſults.

I. The alcohol which had been digeſted on the elaſtic bitumen was not tinged, nor, when ſpontaneouſly evaporated, did it leave any film or ſtain on the glaſs.

II. From aſphaltum, the alcohol had extracted a yellow tincture, which, in ſome ſituations, appeared of a pale olive colour, and, being ſpontaneouſly evaporated, a thick brown liquid was depoſited, in ſmall drops, on the glaſs; theſe drops did not become hard after two months, and poſſeſſed the odour, and every other property, of petroleum. The aſphaltum had loſt in weight about one grain and a half.

III. The cannel coal had communicated a pale yellow tint to the alcohol, which, in the manner above deſcribed, was aſcertained to be cauſed by petroleum; but, from the ſmallneſs of the quantity, the weight could not be determined.

IV. The alcohol which had been digeſted on pit coal, had not aſſumed any colour; but, by ſpontaneous evaporation *, it left a film on the glaſs, which, by its odour, was alſo found to be petroleum.

Alcohol acts ſlightly on bitumens.

By theſe experiments we find, that the action of alcohol on the bitumens is very ſlight; and that the ſmall portion which

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* Spontaneous evaporation, by expoſure to the air, was employed in theſe experiments, for reaſons which muſt be ſufficiently obvious.

may thus be extracted from ſome of them, is petroleum. In theſe, the proceſs of bituminization (if I may be allowed to employ ſuch a term) appears to have been completed, whilſt in the Bovey coal, and eſpecially in the ſubſtance which accompanies it, nature ſeems to have performed only the half of her work, and, from ſome unknown cauſe, to have ſtopped in the middle of her operations. But, by this circumſtance, much light is thrown on the hiſtory of bituminous ſubſtances; and the opinion, that they owe their origin to the organized kingdoms of nature, eſpecially to that of vegetables, which hitherto has been ſupported only by preſumptive proofs, ſeems now, in a great meaſure, to be confirmed, although the cauſes which operate theſe changes on vegetable bodies are as yet undiſcovered.

They are not formed by time alone.

Many facts indicate, that time alone does not reduce animal or vegetable bodies to the ſtate of foſſils. In this country, there are numerous examples of large quantities of timber, (even whole foreſts) which have been ſubmerged prior to any tradition, and which nevertheleſs completely retain their ligneous characters.* Other local cauſes and agents muſt therefore have been required, to form the varieties of coal and other bituminous ſubſtances. In ſome inſtances (as in the formation of Bovey coal), theſe cauſes ſeem to have acted partially and imperfectly, whilſt, in the formation of the greater part of the pit coals, their operation has been extenſive and complete.

In the pit coals, the mineral characters predominate, and the principal veſtige of their real origin ſeems to be bitumen; for the preſence of carbon in the ſtate of oxide, cannot alone be conſidered as deciſive.

Bitumen, therefore, with the exuviæ and impreſſions ſo commonly found in the accompanying ſtrata, muſt be more immediately regarded as the proofs, in favour of the origin of pit coal from organized bodies; and, conſidering the general facts which have been long obſerved, together with thoſe lately adduced reſpecting the Bovey coal, and the ſubſtance which is found with it, we ſeem now to have almoſt unqueſtionable evidence, that bitumen has eſſentially been produced by the modification of ſome of the proximate principles of vegetables, and eſpecially reſin.

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* Phil. Tranſ. for January, 1671. Phil. Tranſ. Vol. XIX. p. 526. Ibid. Vol. XXII. p. 980. Ibid. Vol. XXIII. p. 1073. Ibid. Vol. XXVII. p. 298. Ibid. for 1799, p. 145.

Whether they be of igneous or aqueous formation.

Modern chemiſtry had comparatively made but a ſmall progreſs, when the illuſtrious Bergmann publiſhed his Diſſertation, entitled, *Producta Ignis ſubterranei chemice conſiderata;* for, at that time, the extent and power of chemical action, in the humid way, were very imperfectly underſtood. In that part, however, of the above work, where he ſpeaks of the foſſil wood of Iceland, called Surturbrand, he evidently appears doubtful how far volcanic fire may have acted upon it; although he conceives that, in the formation of it there has been ſome connection with volcanic operations. His words are, "*Quid de ligno foſſili Iſlandiæ ſentiendum ſit, gnaro in loco natali contemplatori* decidendum relinquimus. *Interea, ut cum vulcani operationibus nexum credamus, plures ſuadent rationes, quamvis hucuſque modum ignoremus, quo ſitum texturamque adquiſiverunt hæc ſtrata.*" It certainly was very natural that Bergmann ſhould entertain this opinion, in reſpect to the ſurturbrand; and it is remarkable, that the leaves contained in the ſchiſtus lately deſcribed, are of the ſame nature, and are found in the ſame country. The leaves alſo deſcribed by Mr. St. Fond, are likewiſe found in a country which, according to him, was formerly volcanic. Were theſe ſubſtances, therefore, never found but in countries which either actually are or were volcanic, we ſhould be almoſt compelled to believe, with the Swediſh profeſſor, that the operations of ſubterraneous fires have been concerned in the formation of theſe bodies, or rather in the converſion of them into their preſent ſtate.

They are found in countries evidently not volcanic.

But ſimilar ſubſtances are found in countries where not the ſmalleſt veſtige of volcanic effects can be diſcovered, and Devonſhire moſt undoubtedly is ſuch; yet, nevertheleſs, the Bovey coal is there found ſimilar to the ſurturbrand, in moſt of the external, and (from experiments which I made ſome years ago, I believe I may ſay) chemical properties; to which muſt be added, that both theſe ſubſtances perfectly reſemble each other, by forming regular ſtrata.*

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* Tranſ. of the Linnean Society, Vol. IV. p. 138. Von Troil's Letters, p. 42. *Opuſcula Bergmanni,* Tom. III. p. 239.

Moreover,

Deduction.

Moreover, the half charred appearance of Bovey coal, and of ſurturbrand, cannot be adduced as any proof, that the original vegetable bodies have been expoſed to the partial effects of ſubterraneous fire; for, at this time, we know that the oxidizement of ſubſtances is performed, at leaſt as frequently, and as effectually, by the humid as by the dry way. It would therefore be ſuperfluous here, to enter into an elaborate diſcuſſion, to prove that coal and bitumen, with much greater probability, have been formed without the intervention of fire; and I am the leſs inclined to ſay more upon this ſubject, as I have already publiſhed ſome conſiderations on it in a former paper.*

The new ſubſtance found with the Bovey coal termed *Retinaſphaltum*.

Before I conclude, I muſt beg leave to obſerve, that as the ſubſtance which is found with the Bovey coal is, in every reſpect, ſo totally different from any of the bitumens hitherto diſcovered, it ſeems proper that it ſhould receive ſome ſpecific name; and, as it has been proved to conſiſt partly of a reſin and partly of a bituminous ſubſtance, I am induced to call it *Retinaſphaltum*,† a name by which a full definition of its nature is conveyed.

Account of another ſpecimen, probably of the ſame.

I have lately ſeen, in No. 85 of the *Journal des Mines*, p. 77, an account of a peculiar combuſtible foſſil, found near Helbra, in the county of Mansfield, and deſcribed by Mr. Voight, in his *Verſuch einer Geſchichte der Steinkohle, der Brawnkohle*, &c. p. 188. This ſubſtance is of an aſh-coloured gray, paſſing to grayiſh-white; it is found in a bed of bituminous vegetable earth, which has apparently been produced by the decompoſition of foſſil wood. The pureſt ſpecimens are in the form of nodules; the fracture is earthy; it is opaque; ſoft; brittle; and is very light. When applied to the flame of a candle, it burns and melts like ſealing-wax, at the ſame time diffuſing an odour which is not diſagreeable. This ſubſtance appears to accord in ſo many properties with the retinaſphaltum of Bovey, that I cannot but ſuſpect it to be of a ſimilar nature, and I have little doubt that, by a chemical examination, it will be found to conſiſt partly of reſin and partly of bitumen.

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* Tranſ. of the Linnean Society, Vol. IV. pp. 141, &c.

† From ῥητίνη, reſin; and ἄσφαλτος, bitumen.

VIII.

Account and Deſcription of an improved Air Pump. By Mr. N. MENDELSSOHN.

To Mr. NICHOLSON.

SIR,

ON entering into buſineſs, as a mathematical inſtrument-maker, I reſolved to make it my principal ſtudy to introduce into them all the improvements which the preſent advanced ſtate of ſcience, the nature of the ſubject, and my humble abilities would allow. Thus I have begun with improving *Volta's electrical lamp* and the *air-pump*. I imagine, I have rendered the conſtruction of the latter much more ſimple than it uſually is, and conſequently leſs liable to derangement, at the ſame time that it poſſeſſes a very great exhauſting power. Give me leave, Sir, to offer you a deſcription of this improved air-pump with the requiſite drawings. It is ready for your inſpection. I ſhall think myſelf very happy, if this uſeful philoſophical inſtrument meet with your approbation. And if it be ſo fortunate, I requeſt you the favour of introducing it into public notice, by inſerting my paper in your excellent and deſervedly celebrated Journal.

Introductory letter.

I am, with the higheſt eſteem, Sir,

Your humble and obedient ſervant,

N. MENDELSSOHN.

No. 50, *Surry-Street, Black-Friars,*
Feb. 13, 1805.

Notwithſtanding the many improvements which have been made upon the conſtruction of the air-pump, it was ſtill deſirable that this inſtrument ſhould be more ſimplified in its mechaniſm, and yet poſſeſs the ſame advantages of thoſe conſtructed lately. That the mechanical power of the pump, and not the preſſure of the air, ſhould open the valves, has long been required and already done by Mr. Cuthbertſon, Mr. Haas, and ſeveral other ſkilful artiſts; but may I be allowed to remark, that on reading the deſcription of their inſtruments, they appeared to me to be too compounded. It muſt be very difficult for

Deſcription of Mendelſſohn's improved air-pump.

Deſcription of Mendelſſohn's improved air-pump.

for an experimental philoſopher to clean an inſtrument which, being thus complicated, is not only rendered intricate, but is alſo difficult to be put together again. Being deſirous, therefore, to ſimplify this inſtrument, I adopted the conſtruction here deſcribed, by which it is capable of being put together in leſs than half an hour when cleaned, and requires that operation very ſeldom.

I ſubmit to the judgment of your ſcientific readers how far the preſent inſtrument anſwers its deſired purpoſe. I have rejected that tube which, in common air-pumps, leads from the valves to the receiver, together with the cock that ſerves to ſhut this pipe: the receiver is placed immediately upon the valves, theſe being put on the top of the cylinders, which, conſequently, required the rackwork and pinion to be underneath, and inverted the whole inſtrument. *See the adjoining drawing, Plate* VI. where A B and C D repreſent the two cylinders of glaſs ground and poliſhed inſide. E and F are the two valves that allow the cylinders to communicate with the receiver O through two very ſhort canals A B and C D (*Fig* 2, *Plate* VII) and the cock G. Two other valves that open into the atmoſphere are within the covers *i* and *k*, as may be ſeen in *Fig.* 1, where *e* repreſents one of them. M N is the receiver-plate of glaſs ground flat; P Q a barometer-gauge, upon the plan of the firſt Torricellian tube, as the eaſieſt to conſtruct and the moſt infallible in its effects. It will be found to be here quite out of the way, ſecure from being broke by accident, and the moſt in ſight. H K and I L are two braſs pillars that ſupport the whole. R S V W the uſual rackwork, having a double winch *l m*, which, upon trial, will be found preferable to a ſingle one.

It will now be neceſſary to ſhew how this pump acts, in which it will be ſufficient to explain the action of one cylinder, becauſe the other is in all parts alike. E is a conical metallic valve, from which a canal goes through the cock G up to the receiver, as is ſeen in *Fig.* 1 and 2, *Plate* VII. where all the parts are marked with the ſame letters. E T is a ſteel rod going through a leather box in the piſton U. The top of this rod is fixed to the valve E, and its bottom part ſlides in a ſmall hole with an allowance of 0,1 inch up and downward, conſequently the valve E can move no farther. When the piſton deſcends, it firſt opens the valve by puſhing the rod to the

Deſcription of Mendelſſohn's improved air-pump.

the bottom of the hole. Then it ſlides down along the rod E T, and the air from the receiver has now free acceſs to the cylinder. When the piſton returns it lifts the rod E T, and thus ſhuts up the valve. Then the piſton ſlides again along the rod up to the top of the cylinder, condenſing the air above it, which air, by the leaſt condenſation, opens a valve *e*, *Fig.* 2, and eſcapes freely into the atmoſphere. This laſt valve has neither ſpring or additional weight to ſhut it, but ſhuts by its own weight (about a quarter of an ounce) as ſoon as the piſton is arrived to the top of the cylinder.

The cylinders are made of glaſs, *and the piſtons of tin, ſo well fitted as to be air-tight, without the interpoſition of any leathers.* The friction of theſe two bodies is ſmall beyond expectation, a ſufficient proof that they will be durable. They poſſeſs the further advantage of being capable of ſtanding for even ſix months, after which time they will ſerve without being cleaned or repaired, becauſe they are not liable to be corroded by the oil which they contain, an inconvenience too general in braſs cylinders. After all, if the preſent pump ſhould want cleaning, it is an eaſy operation to take off the top piece *g h*, by unſcrewing the nuts H and I, when this piece, with all the apparatus upon it, will come off. Then each cylinder may very eaſily be ſlid off from the piſton, wiped out and replaced, after having greaſed its inſide with a little of the cleaneſt ſweet oil: The top is then to be put again in its place, and the two nuts H and I being ſcrewed upon it, the inſtrument is ready. Neither racks or pinion need to be taken out of their places, the cylinders ſtanding above them.

The cock is conſtructed ſo, that, being in the ſituation repreſented in *Fig.* 1, the communication is open between the cylinders, the receiver, and the barometer-gauge, and, by a quarter of a revolution, the cylinders are excluded, the receiver and gauge being ſtill left in communication. A little ſtopper in *Fig.* 3, ground into the cock, being open, air is admitted to the receiver if required.

The receiver-plate is of glaſs ground flat, as was mentioned before: this will be found preferable to braſs, becauſe cleaner, and never corroded by acids or water; it will beſides often prove very convenient in making experiments on electricity in the vacuum.

The whole inſtrument is fixed upon a mahogany table, which ſerves as a ſtand to it.

I will conclude by obſerving, that neither the employing of glaſs cylinders, or the method of opening the valves, are new, but, for aught I know, this is the firſt inſtrument of the kind ever executed in this country; as likewiſe the idea of putting the valves at top, and thus ſimplifying the inſtrument, ſeems to have eſcaped the attention of the eminent artiſts both here and abroad; as to my beſt knowledge, it has never been done or deſcribed any where. The metallic piſtons, without leathering, muſt certainly add to the durability, and diminiſh the great labour that uſually attends working an air-pump.

IX.

Letter concerning Palladium, from WILLIAM HYDE WOLLASTON, *M. D. F. R. S. the Diſcoverer of that Metallic Body.*

To Mr. NICHOLSON.

SIR,

The author adverts to former proceedings reſpecting palladium; of which he is the diſcoverer.

THE candour with which you communicated all circumſtances that came to your knowledge concerning palladium, at a time when the diſcoverer of that ſubſtance was yet unknown to you, demands my earlieſt acknowledgments, as having been the author of thoſe communications; and it is proper that I ſhould alſo expreſs the ſatisfaction I received on learning the reſpectable tribunal you nominated at my requeſt, for examining the merits of any attempts that might be made to form that ſubſtance artificially.

Reaſons why the diſcovery was firſt communicated to the world anonymouſly.

As I have already ſhewn (in a paper which you did me the honour to reprint in your Journal for January laſt, p. 34.) by what means a very ſmall quantity of Palladium may be extracted from the ore of platina, and as I have there examined the ſynthetic attempts to prove that this body was a compound, with a degree of attention which I thought due to the chemical ſkill of the perſon who propoſed them, as well as to the degree of uncertainty that muſt attend a ſubject entirely new; I cannot now adduce further *chemical* evidence, and can only add, for the information of thoſe whoſe judgment has been biaſſed by the difficulty of accounting for the production of ſo large a quantity of palladium as was offered for ſale, that a proportional quantity of platina, from which the whole was extracted, was purchaſed by me a few years ſince, with the deſign of rendering it malleable for the different purpoſes to which it is adapted. That object has now been attained, and during the ſolution of it, various unforeſeen appearances occurred, ſome of which led me to the diſcovery of palladium; but there were other circumſtances which could not be accounted for by the exiſtence of that metal alone. On this, and other accounts, I endeavoured to reſerve to myſelf a deliberate examination of thoſe difficulties which the ſubſequent diſcovery of a ſecond new metal, that I have called rhodium, has ſince enabled me to explain, without being anticipated even by thoſe foreign chemiſts, whoſe attention has been particularly directed to this purſuit,

I remain, Sir,
Your obliged and obedient Servant,
W. H. WOLLASTON.

Feb. 23, 1805.

X.

Short Remark on Mr. Walker's laſt Letter reſpecting Focal Images. By C. L.

To Mr. NICHOLSON.

SIR,

I WOULD beg your indulgence for a very ſhort letter, in anſwer to Mr. Ezekiel Walker. The only reply which ſeems needful, on the ſubject of the temper in which he or I may have written, is that if his firſt paper had indicated more of the calm ſpirit of philoſophy, as well as of philoſophical correctneſs, my obſervations might either have been unneceſſary, or, perhaps, drawn up without any extraneous remark. As it is, I do not think myſelf entitled, nor am I indeed inclined to offer any ſtrictures upon his laſt; which I leave to the unbiaſſed judgment of your readers. On the preſent occaſion, I only wiſh to adhere to the experiments. Men of ſcience will find no difficulty in forming a proper eſtimate of our reaſonings. In plain language, therefore, I will beg leave to

C. L. declines any farther diſcuſſion reſpecting Mr. Walker's experiments with lenſes; but denies the facts.

to wave any attention to the hiſtory of Mr. Walker's experiments, upon which I have animadverted, and to deny his facts. What may have deceived him in his proceedings, his admeaſurements, and his repeated regiſterings, is not for me to diſcuſs. If he will ſend his lenſes to you, and you ſhould find that, under any circumſtances whatever of diſtance or poſition, it be poſſible, by a mere alteration in the aperture, to produce a difference, as 2 to 3, in the length of the focal image, (ſee Philoſ. Journal, Vol. IX. p. 165.) I think it will be incumbent upon us to re-examine all the facts and demonſtrations, of what we have hitherto been in the habit of calling the ſcience of optics, in order to reconcile them with ſo ſtrange a reſult.

I am, Sir,
Your obliged
C. L.

XI.

A Communication on the Uſe of Green Vitriol, or Sulphate of Iron, as a Manure; and on the Efficacy of paring and burning depending, partly, on Oxide of Iron. By George Pearson, M. D. *Honorary Member of the Board of Agriculture, F. R. S. From a Communication made by him to the Board, and inſerted in the fourth Volume of their Tranſactions.*

Sulphate of iron or martial vitriol hitherto ſuppoſed deſtructive of vegetation,

I TAKE leave to lay before this Honourable Board, an account of a ſubſtance as a manure, which I find, on examination, is one of the things, hitherto univerſally believed to be a poiſon to vegetables. Having aſcertained that this ſubſtance is what is commonly known by the name of vitriol of iron (the ſulphate of iron of the chemiſts), inveterate opinion prevented me for ſome time from accepting the teſtimony of it as a manure; but feeling the weight of the reſpectable evidence by whom it was atteſted, after conſideration I perceived that the fact in queſtion was not at variance with eſtabliſhed principles of vegetable philoſophy, as I ſhall, I think, make appear in this communication.

—but the contrary is true.

My friend John Williams Willaume, Eſq. of Tingrith in Bedfordſhire, having deſired his brother, Charles Dymoke Willaume, Eſq. to aſk my opinion of a ſaline ſubſtance collected from peat, which has been uſed with profitable conſequences as a manure in his neighbourhood; I propoſed a ſet of queries to Mr. John W. Willaume, the anſwers to which, in the two following copied letters, comprehend the evidence I have to offer.

LETTER No. I.

To Dr. Pearſon, from C. D. Willaume, Eſq.

My dear Sir,

Letter of information concerning peat aſhes or duſt.

I received the incloſed laſt Saturday, and hope the anſwers to your queries will be ſatisfactory, and tend to elucidate this curious ſubject. Though the anſwers under the article *duſt* only relate to your queries, yet my brother has thought proper to advert to the *aſhes*, which you conceive to be a *caput mortuum*; but which have been uſed as, and have been ſuppoſed to be, a beneficial manure from time immemorial. I have reſerved a piece of the peat from which the aſhes are produced, and if you would wiſh to analyſe it, I will ſend it you. Favour me with the reſult of your future inquiries on this ſubject, and I am,

My dear Sir, your's very ſincerely,

C. D. Willaume.

Walham Green,
Aug. 24, 1801.

LETTER No. II.

From John W. Willaume, Eſq. to C. W. Willaume, Eſq.

Queries propoſed by Dr. Pearſon.

Queries reſpecting ſalt of peat.

1. How long has the ſalt of peat been uſed?
2. How much per acre is laid on?
3. On what kind of lands?
4. The effects of it on vegetation?
5. Whether it is mixed with dung manure, or lime?
6. In what parts of the country has it been employed?
7. Any other facts which can be collected relative to the uſe of this ſubſtance?

In anſwering the above queries, I ſhall divide the ſubject into three articles, 1ſt, the *peat* conſidered as an object of fuel; 2d, the *aſhes*; 3d, the *ſalt of peat, or duſt*; the two laſt as objects of manure.

1. Peat

Deſcription of peat.

1. *Peat.* The peat, which is found after the removal of the turf or exterior ſurface, to about a ſpade's depth, has long been known as an article of fuel. It is, however, uſed only by cottagers, who burn it on a brick hearth; it has been rejected from the parlour, the kitchen, the brewhouſe, &c. as being injurious to grates, and to all ſorts of veſſels put on it; it cannot be employed in the roaſting of meat, as it will impart a diſagreeable taſte, and it is deſtructive of all ſorts of furniture by the effluvia which it emits, or by the duſt or aſhes which may chance to be blown from it. If theſe diſagreeable conſequences could be obviated, it might be made an article of general conſumption as a ſubſtitute for coal, much to the advantage of the ſeller and conſumer; it is dug out in the form of a brick to a certain depth, well known to the common labourer. This depth muſt be carefully attended to, leſt you ſhould cut out the ſtaple, in which caſe it would never be retrieved; but, this circumſtance attended to, it will grow again to its former ſtate in the ſpace of fifteen years. Thus the whole moor is divided into proper portions, and periodically cut once in fifteen years.

Its aſhes.

2. *Aſhes.* The turf or ſurface, and ſuch parts of the peat as do not appear to be of the beſt quality, are laid up in conſiderable heaps, and reduced to aſhes by the action of fire. *The aſhes are red.*

Anſwer to Queries.

Anſwers to the queries as to the aſhes.

1. The aſhes have been long known as a manure, and the demand is on the increaſe.

2. The quantity uſually laid on an acre, by ſpreading or ſowing it, is fifty buſhels, either on graſs or arable land.

3. It is laid on hot land. By hot land, we underſtand ſandy, gravelly, chalky ſoils of a dry nature, ſuch as are burnt up on the long continuance of hot weather. It is moſt commonly uſed for graſſes; but is in conſiderable eſteem, as a manure, for oats or barley on land of the nature above-mentioned.

4. The vegetable effect is ſurpriſing, inaſmuch as it will double or treble a crop of any new ſown graſs, ſuch as trefoil, &c. I have ſeen the benefits ariſing from it on old paſture land much overgrown with moſs, which it effectually deſtroys, and produces in its ſtead white or Dutch clover. You may

trace to an inch the ceſſation and recommencement of this manure. It is obſervable, that near the fire heaps, as far as the wind can carry the lighter parts of the aſhes, the production of clover is ſure to be abundant; it is equally favourable to the growth of barley or oats.

5. It is not mixed with lime, or any other manure.

6. Theſe aſhes are bought by a ſet of higlers, who carry them in bags loaded on aſſes to a conſiderable diſtance, where they are known to be in great repute; they muſt come exceſſively dear to the conſumer by this mode of conveyance. The farmers in the vicinity ſend for them in waggons, particularly Mr. Brumiger, near Sundon in Bedfordſhire, a conſiderable and intelligent farmer, who increaſes his conſumption every year, both for his graſs and arable land.

3. *The Salt of Peat, or Duſt.*

Anſwers reſpecting the ſalt of peat.

Anſwer to Queries.—1. The duſt or gray ſaline ſubſtance is produced by beating the earth containing this ſalt to a powder; it is found in particular ſpots, not univerſally, the earth not being equally impregnated with it in all places; it has not been known as a manure above ſix years; but on trial greatly increaſes in reputation and demand.

2. Fifty buſhels are the proper quantity per acre. This ſhould not be exceeded, for if it be laid on in too great abundance, it may prove extremely deleterious.

3. It is uſed for cold lands. By cold lands we underſtand clayey, or any wet grounds.

4. It will much improve the vegetation of ſowed graſſes, and old paſture, and is equally favourable to the production of corn; the ground, whether graſs or arable, being of a cold nature.

5. It is not mixed with lime, or any other ſubſtance.

6. The duſt is likewiſe bought by the higlers, and carried to great diſtances. The nearer farmers likewiſe ſend for the duſt in waggons, particularly Mr. Anſtie, of Dunſtable Houghton, and Mr. Smith, of Sundon, who hold this manure in great eſteem.

Your's, &c.

Tingrith, Aug. 19, 1801. J. W. Willaume.

Dr. Pearſon's Experiments, Obſervations, and Remarks on the Subſtance called Salt of Peat, or Duſt.

Dr. Pearſon's chemical examination of ſalt of peat or duſt.

1. It is a blackiſh gray, coarſe, and rather heavy powder. Has no ſmell; taſtes ſtrongly ſtyptic; readily diſſolves in the mouth; did not deliqueſce on expoſure to the air.

2. Diſſolves in four times its weight of water of the temperature of ſixty degrees of Fahrenheit, and in twice its weight of boiling hot water, giving a pale green coloured ſolution, with a trifling ſediment, which is inſoluble in muriatic acid.

3. To the ſolution (2.) I added a little liquid pruſſiate of vegetable alkali in a perfectly neutral ſtate, which occaſioned immediately a moſt abundant precipitation of pruſſiate of iron; and this teſt was added gradually, till no further precipitation took place.

4. Into the decanted and filtrated fluid (3) was poured liquid cauſtic volatile alkali, but without inducing any change.

5. Into the ſame fluid (3) was poured liquid carbonate of vegetable alkali, which produced a ſcarcely perceivable cloudy appearance.

6. Into the ſolution (3) was dropped the aqueous ſolution of muriate of baryt, which occaſioned immediately a milky appearance.

7. To the ſolution (3) I added the oxalic acid, and turbidneſs enſued.

8. A little of the powdery ſubſtance, called the ſalt of peat, with concentrated ſulphuric acid, produced no emiſſion of fumes, nor ſmell.

9. The ſolution (2) with muriate of baryt, immediately grew thick and white as cream.

10. The ſolution (2) with carbonate of potaſh, depoſited a very copious greeniſh ſediment; and the ſame effect enſued with cauſtic volatile alkali.

11. The ſolution (2) with oxalic acid, gave inſtantly a very turbid bluiſh green precipitation.

The preceding experiments manifeſted that the *peat ſalt* conſiſts of *ſulphate of iron,* vulgarly called green vitriol of iron, mixed with a very minute proportion of ſilicious earth, and of lime united either to ſulphuric acid, or to carbonic acid. But the preſence of the earths magneſia and argill; the uncombined alkalies; the uncombined acids; are by theſe experiments excluded. In ſhort, the ſalt of peat is almoſt pure *ſulphate of iron.*

Remarks.

1. The ſalt of peat is, I apprehend, depoſited by evaporations which run over the moors, where it is found; and hence I ſhould expect many of ſuch waters to be ſtrongly impregnated with it, and in many parts the ſoil to be tinged red and yellow by ochre. Very likely * on enquiry much iron pyrites will be found on, or near the moors.

Salt of peat how produced.

2. The quantity ſpread on land is ſaid to be fifty buſhels per acre, which I eſtimate at 2,250 pounds avoirdupoiſe; this will give near ſeven ounces and a half per ſquare yard. If a larger quantity be applied, it is obſerved it will prove extremely deleterious. This is true alſo of every other manure, ſuch as lime, alkaline ſalts, marine ſalt, nay, of the dung of animals; for if they be uſed in certain quantities, they *poiſon* plants, inſtead of promoting their growth. This is equally true in the animal kingdom; for there is not an article taken as food, or as ſeaſoning, which is not a poiſon, if taken in certain quantities. A human creature may be poiſoned or alimented by beef or pudding, according to the quantity of them taken into the ſtomach. He may be poiſoned, or have digeſtion greatly aſſiſted by ſalt, or pepper, according to their quantity. In brief, the vulgar notion of the term *poiſon* is erroneous: for by it is conceived that ſubſtances ſo called are in their nature poſitively deſtructive of life; but the truth is that the moſt virulent poiſons are, in all reaſon and fact, only deleterious according to the quantity applied. White arſenic ſwallowed in the quantity of ten grains or leſs, will deſtroy life; but in the quantity of one-ſixteenth of a grain, it is as harmleſs as a glaſs of wine; and further, in that doſe is a remedy for inveterate agues.

Its quantity muſt be regulated as a manure:

and it is hurtful only by exceſs, as food and condiments alſo are.

From theſe conſiderations I conclude, that there is no admiſſible contradictory evidence to the teſtimonies for the fer-

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* "This is," ſays Mr. Willaume, "exactly the fact. This ſulphate of iron, the ſalt of peat, during the heat of the ſummer is frequently found in a chryſtalized ſtate, very white, and crackling under the feet; but is deliqueſcent in that form, and turns to its former dark colour when the air becomes moiſt."—*Note by Mr. J. W. Willaume.*

tillizing

tilizing effect of ſulphate of iron, unleſs by ſuch contravening evidence the quantity ſtated to be uſed exceed fifty buſhels per acre; it being an eſtabliſhed fact, that in certain proportions this metallic ſalt is a poiſon to plants.

This diſcovery of Mr. Willaume will, I think, give new light, ſo as to explain fully the *rationale* of the improvement of land by the burnt earth and aſhes from paring and burning. It is uſual to account for the effects of this proceſs, by referring to ſuppoſed alkaline or other ſalts; but of theſe there is no evidence, nay, on trial I have not detected them, or at leaſt not in any efficient quantity; but this I know, that ſuch earth and aſhes contain *oxide of iron*, and as I ſuſpect of *manganeſe;* which from the analyſis, and the effect of ſalt of peat, muſt now be admitted into the claſs of manures. This very communication of Mr. Willaume, affords evidence of the truth of this conjecture, for the *aſhes of the peat which afford the ſalt* "have been long known as a manure, and the demand is on the increaſe:" of courſe, theſe aſhes contain an unuſual quantity of oxide of iron. A conſequence of this reaſoning is, that the burnt earth of ſoils will, *cæteris paribus*, fertilize in proportion to the oxide of iron it contains. Accordingly the aſhes of the peat, ſays Mr. Willaume, have a ſurpriſing effect, they "will double or treble a crop of any new-ſown graſs, ſuch as trefoil, &c." they are ſo beneficial, that in ſpite of the expence they are carried in bags by higlers to great diſtances. It would be extending this paper beyond the propoſed limits, to reaſon at a greater length, and to make a further induction of facts: therefore I will cloſe with aſſerting, that the more I contemplate the facts in Mr. Willaume's letter, the more evidence I perceive for the truth, that metallic ſalts, and metallic oxides in general, and ſalts and oxides of iron in particular, are manures, if applied in proper doſes.

It is conſidered as effective upon the ſame principle as condiments or ſeaſoners.

I do not think it is within the deſign of this paper to make obſervations on the anſwers to the 2d, 3d, 4th, 5th, and 6th queries, except, once for all deſiring that it may be underſtood, that I conſider the *ſalt of peat and the aſhes of peat*, as operating in promoting vegetation analogous to ſeaſoning, or condiments, taken with the food of animals; that is, analogous to muſtard, cinnamon, ginger, &c. which are not of themſelves at all or neceſſarily nutritious, but contribute to render other things nutritious, by exciting the action of the ſtomach and other organs of digeſtion and aſſimulation. I have no doubt of the truth of the propoſition, that no living thing, neither plant nor animal, can grow and live in a ſtate of viſible action without conſtant ſupplies of *matter which has been alive;* in other words, *living* animals and vegetables can only live on *dead* animals and *dead* vegetables. No plant, nor animal has ever been known by experience, nor in the nature of things does it ſeem reaſonable, that they can be nouriſhed by mere water and pure air, as ſome perſons have aſſerted.

I ſhall make a few remarks on the other *two ſubſtances* which are the ſubject of Mr. Willaume's letter.

2. *The Peat.*

Chemical remark on peat.

The peat is a denſe maſs of vegetable matter for a certain depth, partly in a *dead* and partly in a *living* ſtate, with which is mixed more or leſs earth, and in burning it affords ſo much empyreumatic oil, as to give a diſagreeable taſte to roaſted proviſions; hence, as we are told, it has been rejected from the kitchen. This fuel affords a vaſt quantity of what the chemiſts call *lignic acid;* hence it is rejected alſo from the parlour, as very deſtructive to the grates. I beg to ſuggeſt that this lignic acid might be ſaved in burning the peat as fuel, and be uſed for various purpoſes in manufactures; and the charred peat may be uſed in place of charcoal of wood. Probably too other uſeful products will be found, on examining the matters more accurately which are afforded by diſtillation.

3. *Aſhes.*

The aſhe peat

If the peat were mere vegetable matter, the aſhes afforded by it would be as trifling as thoſe of wood; but ſome parts of the moor contain ſo much earth and oxide of iron, as to leave behind, on burning, a conſiderable quantity of incombuſtible matter; and ſuch kind of peat, we are told, is not uſed as fuel; but, after burning, the reſiduary matter is an efficacious manure, much more ſo than is commonly afforded by paring and burning. The aſhes are more red and more fertilizing than aſhes of common turf, becauſe they contain more iron.

produce clover.

The ſpontaneous ſpringing up of white clover, in land manured with theſe aſhes, is ſimilar to the ſpontaneous growth of this plant on heath land, which has been covered with lime to deſtroy all its preſent vegetation; and this fact ſhews that probably

Explanation.

bably theſe are ſeeds buried in the earth for many ages, which yet remain alive, but do not grow until expoſed to the ſtimuli of air, water calorific, and lifeleſs animal, or vegetable matter.

(To be concluded in our next.)

XII.

Letter from Mr. ACCUM, *reſpecting an Error ſtated to exiſt in his Practical Chemiſtry.*

To Mr. NICHOLSON.

SIR,

Compton-Street, Soho,
Feb. 16, 1805.

Omiſſion ſtated to be made in Accum's chemiſtry.

W. F. C. cenſures me in your Journal, No. 38, page 105, of not having given in my book on Practical Chemiſtry "*The means of preparing either nitric acid, or nitrate of potaſh, or rather having ſtated them by implication, as incapable of being produced by art.*" The fidelity with which you have laid his remarks before the public, encourages me to hope, that with equal impartiality you will allow me to appear before the ſame tribunal, in order to plead to this accuſation NOT GUILTY. For the method of obtaining nitric acid from its conſtituent principles, W. F. C. may read in the Vol. I. page 211.

In order to let the judicious readers who are not in poſſeſſion of the work, judge for themſelves, I beg leave to lay before them the method there pointed out, which literally runs thus:

Paſſage where the ſuppoſed omiſſion is found.

"*Take a barometer tube, the diameter of which is about $\frac{1}{8}$ part of an inch. Shut one of its extremities with a cork, through the middle of which paſſes a ſmall wire with a ball of metal at each end. Fill the tube with mercury and invert it into a baſon of this fluid. Throw up into this tube as much of a mixture of 12 parts of nitrogen gas, and 87 parts of oxigen gas as will fill 3 inches. Through this gas by means of the wire in the cork paſs a number of electric ſparks; the volume of the gas gradually diminiſhes, and in its place will be found nitrous acid.*" *

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* *Nitrous acid* does not differ from *nitric acid*, in compoſition, but merely by the admixture of nitrous gas. See the book under conſideration, Vol. II. p. 281.

It appears that W. F. C. has not noticed theſe lines. The production of nitric acid by ſyntheſis, being here clearly ſtated, his animadverſion relative to that ſubject falls to the ground.

Other explanations.

To exculpate myſelf from his further accuſation, namely, of not having noticed the production of nitrate of potaſh by artificial means, or rather having ſtated it by implication, will become equally erroneous, on reminding him, that the method of preparing this ſalt artificially is pointed out, Vol. II. page 287, thus, "*Nitrate of potaſh may be prepared by neutralizing carbonate of potaſh with nitric acid.*"

From what has been ſtated, it is obvious that W. F. C.'s remarks are not correct, for both the methods of obtaining nitric acid, and nitrate of potaſh are correctly given, and not by implication, as W. F. C. apprehends.

I have the honour to be, Sir,
Your's,
FREDERICK ACCUM.

XIII.

Proceſſes for obtaining a durable and ſuperior Lake from Madder.

By Sir H. C. ENGLEFIELD, *M. P. F. R. S.**

Introduction.

THE want of a durable red colour, which ſhould poſſeſs ſomething of the depth and tranſparency of the lakes made from cochineal, firſt induced me to try whether the madder root, which is well known to furniſh a dye leſs ſubject to change by expoſure to air, than any other vegetable colour, except indigo, might not produce ſomething of the colour I wanted.

Madder lakes have been already uſed; but they were bad.

Several of the moſt eminent painters of this country have, for ſome time, been in the habit of uſing madder lakes in oil pictures: but the colours they poſſeſſed under this name were either a yellowiſh red, nearly of the hue of brick-duſt, or a pale pink opake, and without clearneſs or depth of tint, and quite unfit to be uſed in water-coloured drawing, which was the principal object of my ſearch.

Margraf's proceſs affords a weak colour,

My firſt attempts were to repeat the proceſs given by Margraf, in the Memoirs of the Academy of Berlin: but the colour

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* From the Tranſactions of the Society of Arts, who voted the gold medal in honour of this diſcovery. 1804.

lour produced by this mode was of a pale red, and very opake, although the eminent author of the proceſs ſtates the colour he produced to be that of "*le ſang enflummé*," which probably means a deep blood colour. It may, however, be obſerved, that colours prepared with a baſis of alumine will appear much deeper when ground in oil than they do in the lump, the oil rendering the alumine nearly tranſparent. This advantage is however, loſt in water-colours. On examining the reſiduum of the madder root, after it had been treated in Margraf's method, it appeared tinged with ſo rich a red, that it was obvious, that by far the greater part of the colour ſtill remained in it, and that the moſt powerful and beautiful part. To extract this, ſeveral ineffectual trials were made, which it would be uſeleſs to enter into; but, on attentively examining the appearances which took place on infuſing the madder in water, I began to ſuſpect that the red colouring matter was very little, if at all, ſoluble in water, and that it was only mechanically mixed with the water when poured on the root, and ſuſpended in it by the mucilage, with which the root abounds.

becauſe moſt of the colour remained in the root.

Water takes up little of the colour,

A very ſmall quantity, therefore, can be obtained by any infuſion or decoction, as the greater part ſinks down on the root, or remains with it on the ſieve, or in the bag, through which the infuſion or decoction is paſſed to render it clear. I therefore was induced to try whether, by ſome merely mechanical means, I could not ſeparate the colouring matter from the fibrous part of the root. In this attempt my ſucceſs was fully equal to my hopes; and, after ſeveral trials, I conſider the proceſs I am now about to deſcribe, as the moſt perfect I have been able to diſcover.

Whence mechanical preſſure is adviſable.

Proceſs 1. Two ounces of madder were pounded in a bag with a pint of water;

Proceſs 1. Encloſe two ounces, troy weight, of the fineſt Dutch madder, known in commerce by the name of crop madder, in a bag, capable of containing three or four times that quantity, and made of ſtrong and fine calico. Put it into a large marble, or porcelain mortar, and pour on it about a pint of cold ſoft water. The Thames water, when filtered, is as good as can be uſed; it being very nearly as pure as diſtilled water, at leaſt when taken up a very little way above London. With a marble or porcelain peſtle, preſs the bag ſtrongly in every direction, and, as it were, rub and pound it as much as can be done without endangering the bag. The water will very ſoon be loaded with the colouring matter, ſo as to be quite opake and muddy. Pour off the water, and add another pint of freſh water to the root, agitating and triturating it in the manner before deſcribed; and repeat the operation till the water comes off the root very ſlightly tinged. About five pints of water, if well agitated and rubbed, will extract from the root nearly the whole of its colour; and if the reſidual root be taken out of the bag and dried, it will be found to weigh not more than five drachms, apothecaries weight; its colour will be a kind of light nankeen, or cinnamon, and it will have entirely loſt the peculiar odour of the root, and only retain a faint woody ſmell.

The water when turbid with colour is poured off and another pint added; and rubbed as before:

Five ſucceſſive pints extract all the colour, leaving five drams of fibre.

The water loaded with the colouring matter, muſt be put into an earthen or well-tinned copper, or, what is ſtill better, a ſilver veſſel, (for the uſe of iron muſt be carefully avoided through the whole), and heated till it juſt boils. It muſt then be poured into a large earthen or porcelain baſon, and an ounce troy weight of alum diſſolved in about a pint of boiling ſoft water, muſt be poured into it, and ſtirred until it is thoroughly mixed. About an ounce and a half of a ſaturated ſolution of mild vegetable alkali ſhould be gently poured in, ſtirring the whole well all the time. A conſiderable effervescence will take place, and an immediate precipitation of the colour. The whole ſhould be ſuffered to ſtand till cold; and the clear yellow liquor may then be poured off from the red precipitate. A quart of boiling ſoft water ſhould again be poured on it, and well ſtirred. When cool, the colour may be ſeparated from the liquor by filtration through paper in the uſual way; and boiling water ſhould be poured on it in the filter, till it paſſes through of a light ſtraw colour, and quite free from any alkaline taſte. The colour may now be gently dried; and when quite dry, it will be found to weigh half an ounce; juſt a fourth part of the weight of the madder employed.

The coloured water is made boiling hot;

and one oz. of alum diſſolved in a pint of boiling water added;

$1\frac{1}{2}$ oz. of mild veg. alk. afford a precipitate of colour.

Edulcoration, filtering and drying afford half an ounce of lake,

By analyſis, this colour poſſeſſes rather more than 40 per cent. of alumine. If leſs than an ounce of alum be employed with two ounces of madder, the colour will be rather deeper; but if leſs than three quarters of an ounce be uſed, the whole of the colouring matter will not be combined with alumine. On the whole, I conſider the proportion of an ounce of alum to two ounces of madder, as the beſt.

which contains two fifths of alumine.

Proceſs 2. If, when the ſolution of alum is added to the water loaded with the colouring matter of the root, the whole be

Proceſs 2. If the liquor of proceſs 1. be ſuffered to cool and ſettle after the alum was added it affords a dull red ſediment. and the remaining fluid if heated and precipitated by alkali, affords a fine but not ſolid lake. This is Watt's proceſs.

be ſuffered to ſtand, without the addition of the alkali, a conſiderable precipitation will take place, which will be of a dark dull red. The remaining liquor, if again heated, will, by the admiſſion of the alkali, produce a roſe-coloured precipitate of a beautiful tint, but wanting in force and depth of tone.

This is the proceſs recommended by Mr. Watt, in his Eſſay on Madder, in the *Annals de chymie, Tome* 7; and this latter colour is what may perhaps, with propriety, be called Madder Lake. But, although the lighter red may be excellent for many purpoſes, yet I conſider the colour produced by the union of the two colouring matters, as given in the firſt proceſs, as far preferable for general uſe, being of a very beautiful hue when uſed thin, and poſſeſſing unrivalled depth and richneſs either in oil or water, when laid on in greater body.

Leſs alum cauſes the ſecond precipitate to be leſs in quantity, but richer.

If but half an ounce of alum be added to the two ounces of the root, the firſt precipitate will be nearly ſimilar to that when an ounce is employed; but the ſecond, or lake precipitate, will be leſs in quantity, and of a deeper and richer tint. In this caſe the whole of the colouring matter, as before obſerved, is certainly not combined with the alumine; for, on adding more alum to the remaining liquor, a precipitate is obtained of a light purpliſh red. In this proceſs, when two ounces of madder and an ounce of alum are uſed, the firſt precipitate has about 20 per cent. of alumine, and the ſecond, or lake precipitate, about 53 per cent; but theſe proportions will vary a little in repetitions of the proceſs.

Proceſs 3. When the waſhing is performed by boiling inſtead of cold water, the colour is neither ſo good nor ſo plentiful.

Proceſs 3. If the madder, inſtead of being waſhed and triturated with cold water, as directed in the foregoing proceſs, be treated in exactly the ſame manner with boiling water; the colour obtained will be rather darker, but ſcarcely of ſo good a tint; and the reſiduum of the root, however carefully preſſed and waſhed, will retain a ſtrong purpliſh hue; a full proof that ſome valuable colour is retained in it, probably fixed in the woody fibre by the action of heat. Mr. Watt, in his excellent treatiſe on madder above mentioned, obſerves, that cold water extracts the colour better than hot water; and I have reaſon to ſuſpect, that a portion of that colouring matter, which produces the bright red pigment, diſtinguiſhed before by the name of Madder Lake, remains attached to the root, when acted on by boiling water.

Proceſs 4. If to two ounces of madder, a pint of cold water be added, and the whole be ſuffered to ſtand for a few days (three or four days) in a wide-mouthed bottle, lightly corked, in a temperature of between 50° and 60°, and often ſhaken; a ſlight fermentation will take place, the infuſion will acquire a vinous ſmell, and the mucilaginous part of the root will be in a great degree deſtroyed, and its yellow colour much leſſened. If the whole be then poured into a calico bag, and the liquor be ſuffered to drain away without preſſure, and then the root remaining in the bag be treated with cold water, &c. exactly as directed in the firſt proceſs, the red colouring matter will quit the root with much greater eaſe than before fermentation. It will alſo be equal in quantity to that afforded by the firſt proceſs, but of a much lighter red. This difference of tint appears to be owing to a deſtruction of a part of the lake by the fermentation of the root; for if the colours from the fermented root be obtained ſeparate, as in Proceſs 2, the firſt precipitate will not ſenſibly differ from that obtained from the unfermented madder, but the ſecond, or lake will be of a very light pink. This proceſs, then, is not to be recommended.

Proceſs 4. If the madder be ſuffered to ſtand before waſhing for ſome days in cold water, the mucilage is deſtroyed by fermentation, and the colour more eaſily extracted; but it is not ſo good.

Spaniſh and Smyrna Madders.

Spaniſh Madder affords a colour of rather a deeper tone than the Dutch Madder, but it does not appear to be of ſo pure a red as the Zealand Crop Madder.

The Smyrna Madder is a very valuable root. The colour produced from it by Proceſs 1, is of a deeper and richer tint than any I have obtained from the Dutch Madder. The quantity produced from two ounces, is only three drachms, twenty-four grains: but this is not to be wondered at; for as this madder is imported in the entire root in a dry ſtate, and the Crop Madder of Zealand conſiſts principally of the bark, in which probably the greateſt part of the colouring ſubſtance reſides, there is every reaſon to think that the Smyrna madder really contains a greater proportion of colour than the Zealand, in equal weights of the entire root.

Spaniſh madder affords a deeper but leſs pure colour than Dutch madder: Smyrna madder is better than Dutch.

The products of Proceſs 2 prove, that the *lake* of the Smyrna madder is more abundant in quantity, and of a richer tone than that of the Dutch root; for, from two ounces of Dutch madder the firſt precipitate was two drachms, and the lake was two drachms and forty eight grains; whereas, from two ounces of the Smyrna root the firſt precipitate was one drachm and twenty four grains, and the lake was two drachms and twenty-four grains. The proportion of the lake to the other colour is, therefore, much higher in the Smyrna, than in the Dutch root.

Freſh Madder.

The recent root is preferable.

The colour may be prepared from the recent root; and it will be of a quality equal, if not ſuperior, to any other. The difficulty of procuring the freſh root has prevented me from making as many experiments on it as I could have wiſhed. I procured, however, a ſmall quantity of the beſt roots packed in moſs from Holland, and the following proceſs anſwered perfectly well.

Experiment according to proceſs 1. with freſh madder;

Eight ounces of the root having been firſt well waſhed and cleaned from dirt of all kinds, were broken into ſmall pieces, and pounded in a bell-metal mortar, with a wooden peſtle, till reduced into an uniform paſte. This paſte being encloſed in a calico bag, was waſhed and triturated, as deſcribed in the firſt proceſs, with cold water. About five pints ſeem to have extracted nearly the whole of the colour. To the water thus loaded with colour, and boiled as before, one ounce of alum, diſſolved in a pint of boiling water, was added, and the alkali poured on the whole, till the taſte of the mixture was juſt perceptibly alkaline. The colour thus obtained, when dry, was of a very beautiful quality.

very ſucceſsful.

It promiſes great advantage to the arts.

The ſucceſs of this experiment, which was twice repeated with the ſame reſult, has led me to hope, that it is not impoſſible that the mode of obtaining the colour from the freſh root here deſcribed, may be productive of advantages for more extenſive uſe than I had in view when firſt I attempted to obtain a pigment from madder. Many tracts of land in this country are as well adapted to the growth of this valuable article, as the ſoil of Holland can be; and the cultivation of it, which has more than once been attempted to a conſiderable extent, has been laid aſide, principally from the expence attendant on the erection of drying-houſes and mills, and the great expence and nicety requiſite for conducting the proceſs of drying. But ſhould the colour prepared in the mode juſt deſcribed, be found to anſwer the purpoſes of the dyers and calico-printers, the proceſs is ſo eaſy, and the apparatus required

For if the colour ſhould anſwer for dying, &c. it would ſave drying and carriage;

quired for it ſo little expenſive, that it might be in the power of any grower of the root to extract the colour: beſides which, another great advantage would be obtained; the colour thus ſeparated from the root, may be kept any length of time, without danger of ſpoiling, and its carriage would be only one fourth of that of the root. I am, moreover, thoroughly inclined to believe, that in the preſent mode of uſing the root, a very conſiderable part of the colour is left in it by the dyers; and, ſhould this prove to be the caſe, an advantage much greater than any hitherto adverted to, may ariſe from the proceſs here recommended.

may be kept for a long time,

and probably be more effective.

Should it be attempted to obtain the colour from the freſh root, on an extenſive ſcale, I ſhould recommend, that the root be firſt reduced to as uniform a pulp as poſſible, by grinding or pounding. To this purpoſe, it is probable that the cyder-mill would anſwer perfectly well; and its extreme ſimplicity is a great recommendation. For the purpoſe of trituration, bags of woolen, ſuch as are uſed in the oil-mills, would probably anſwer as well as calico, and they would be much cheaper and more durable. A large vat, with ſtampers, would be eaſily conſtructed, by thoſe who are converſant in mechanics, for the holding them and preſſing them in water; and when the colour was boiled and precipitated, the flues of the boilers might eaſily be formed into convenient drying tables, without any additional expenſe of fuel. The part of the proceſs, which I conſider as of the greateſt importance, and as being the eſſential advantage of my methods over all thoſe which have come to my knowledge, is the trituration or preſſing of the root in water; and I believe that the colouring matter of the root has not been hitherto conſidered as ſo nearly inſoluble in water, as I have reaſon to think it is.

Directions for eſtabliſhing a manufactory of madder colour.

It were much to be wiſhed, that in the preſent advanced ſtate of Chemiſtry, ſome ſkilful analyſer would inveſtigate the properties of this very uſeful root; in which perhaps it will be found, that there are three, if not four, different colouring ſubſtances. Such are the proceſſes and views, which I have thought it not improper to ſubmit to the conſideration of the Society of Arts, &c.

We want a good analyſis of madder.

I have only now to deſcribe the ſpecimens which accompany this paper; aſſuring the Society, that they have been all prepared by my own hands entirely, and that I am therefore reſponſible

The ſpecimens exhibited to the Society.

reſponſible for their having been produced by the proceſſes ſtated, without the addition of any foreign matter whatever, excepting the cake ground up with gum, and the bladder of oil-colour, which were prepared from the colour which I gave him, by Mr. Newman, of Soho-Square, whoſe ſkill and fidelity are too well known to need any teſtimony in their favour.

It may be proper to add, that all the colours produced from the Dutch madder were prepared from the ſame parcel of crop madder, in order that the differences in them might proceed from the proceſſes, and not from a variation in the qualities of the root, which, in different ſpecimens, will produce different ſhades of colour under the ſame mode of treatment.*

1. Dutch madder, treated by proceſs 1ſt.
2. Ditto - - - - proceſs 2d.
3. Ditto - - - - proceſs 3d.
4. Ditto - - - - proceſs 4th.
5. Dutch madder, two ounces; alum, half an ounce; treated by proceſs 2.
6. Dutch madder, two ounces; alum, one ounce; fermented two days, and then treated by proceſs 2.
7. Produce of proceſs 1, ground in gum by Mr. Newman.
8. Produce of proceſs 1, ground in oil by Mr. Newman.

S—1. Smyrna madder, by proceſs 1.
S—2. Ditto - - - proceſs 2.
S—3. Ditto - - - proceſs 3.
S—4. Ditto - - - proceſs 4.

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* Certificates accompanied the foregoing deſcription, from Mr. Cotman and Mr. Munn, teſtifying the merits of Sir H. Englefield's madder lakes, as water colours; and alſo, from Meſſrs. Weſt, Trumbull, Opie, Turner, Daniel, and Hoppner, ſpeaking greatly in its favour, where it has been tried in oil-colours.

The

XIV.

The Dutch Method of curing Herrings, extracted and tranſlated from the German of Krünitz's Economical Encyclopædia (Oeconomiſche Encyclopädie), *Article Hüring, by* J. Hinckley, *Eſq. F. S. A.**

Dutch method of catching and curing of herrings.

THE veſſels employed in this fiſhery, commonly called herring-buſſes, from the Dutch name, are generally between 48 and 60 tons burthen, though ſome from 40 to 80 and 100 tons are uſed. The largeſt of all carry 120 tons, are three-maſted veſſels, with one deck, and a cabin at each end; that a-head ſerving as a kitchen. Of the larger, the crews are 24 men, thoſe of the ſmaller 18. They carry a few ſmall guns and muſquetry.

Their nets are dipped, or caſt out, in the evening, and drawn up in the morning. It requires three hours to wind them on board. From the net, the fiſh are immediately put into baſkets, while others of the crew are occupied till evening in gutting, ſalting, and packing. But although from ten to fifteen laſt are ſometimes taken at a draught, the twelve perſons uſually employed for the purpoſe, cannot complete more than five laſt in a day.

During the three firſt weeks, from the 25th of June to the 16th of July, all the freſh-caught herrings are thrown into caſks without picking, and conveyed to Holland, in the jägers, or yachts †, that accompany the herring-buſſes. But, after this period, immediately on being got on board and gutted, they are aſſorted into three qualities,—maiden herrings,

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* From the Tranſactions of the Society of Arts, who have publiſhed it for the information of our fiſhers in North Britain, who appear to be well acquainted with the treatment from catching and landing, but not with the ſubſequent proceſſes. Mr. Walter Baine, of Greenock, had the Society's ſilver medal for this object laſt ſeaſon.

† Theſe are ſmall faſt-ſailing veſſels, which follow the herring-buſſes, ſupply them with proviſions, caſks, ſalt, and other neceſſaries, and carry the fiſh that have been taken to the neareſt port, where they are re-packed, and prepared for ſending to the places of their deſtination.

Dutch method of catching and curing of herrings.

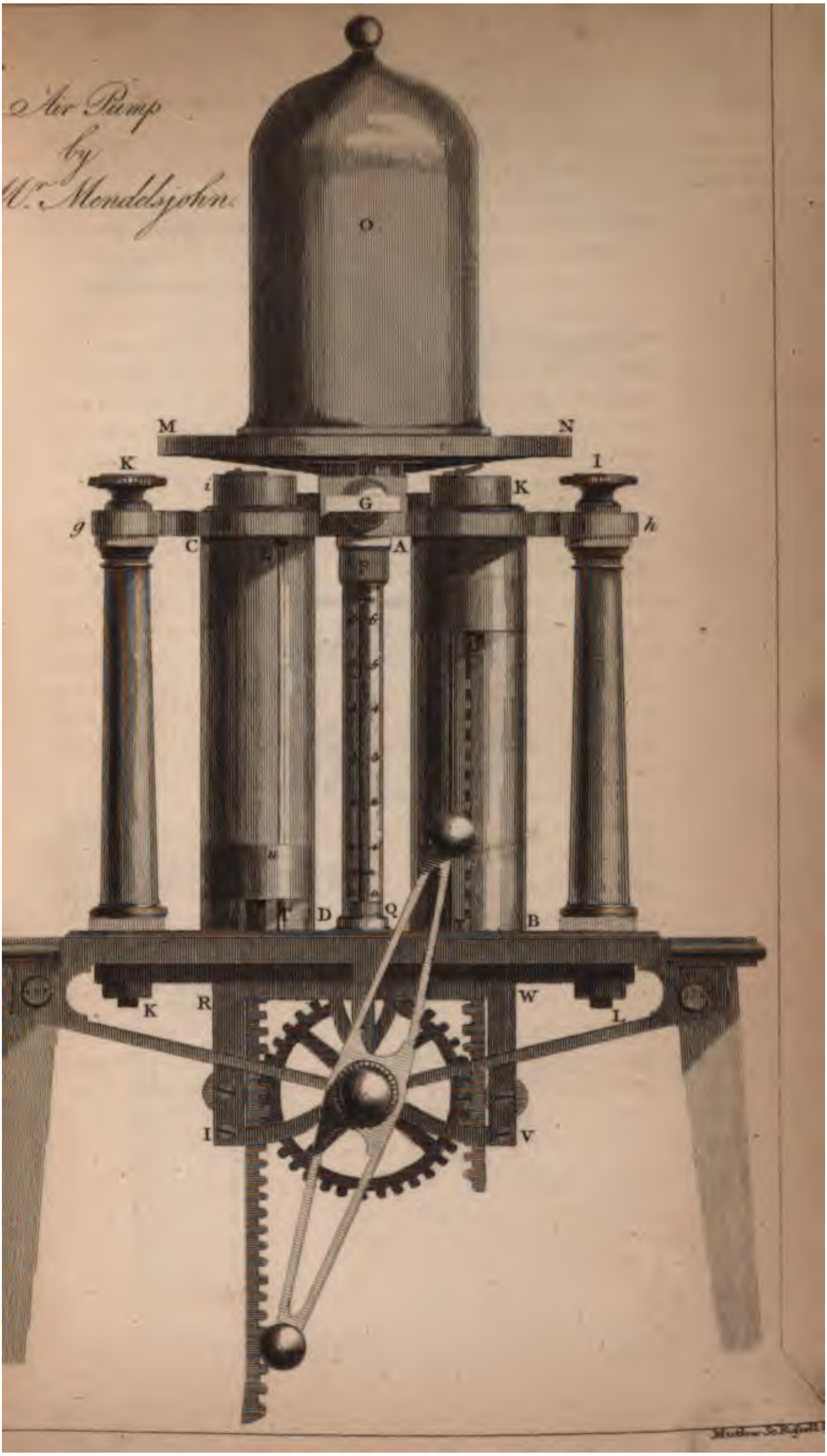
rings, full herrings, and ſhot herrings. The firſt of theſe are thoſe taken earlieſt, and without row or melt, but which, though well flavoured, do not keep. Full herrings are thoſe taken at Midſummer, on the point of ſpawning; from which the brand-herrings, ſo called from the barrels being marked with a hot iron, only differ in being caught later, re-packed immediately on arrival, and ſo cloſe and hard preſſed down, that they do not require re-packing at other places, but only new pickle; and are immediately expedited, or may remain on hand: whereas the other two ſorts, not being ſo cloſely laid, muſt abſolutely be re-packed. Shot-herrings are thoſe which have ſpawned, or are taken in the act of ſpawning, in conſequence of which they are thin and lean.

With the laſt two ſorts the buſſes themſelves return (as ſoon as they have got their loading, or find no more fiſh), one after another, to port, where all three ſorts, except the brand-herrings, before being expedited, are opened, ſalted anew, re-packed, and ſo heaped up, that fourteen caſks are re-packed in twelve, which make a laſt. By a regulation of the States-General, this re-packing muſt be performed in the open air, where ſtrict watch is kept, that the ſpoiling fiſh be carefully ſeparated from the good, and the latter properly laid in the barrels, and ſtrongly preſſed down.

The Dutch fiſhery continues generally from twenty to twenty-ſix weeks, or even ſomewhat longer, namely, from the 25th of June to the middle of January. The Dutch fiſh only on the Scotch and Engliſh coaſts, off Hittland, Fairhill, and Bocken, from Midſummer till the 25th of July; off Bocken or Serenial, from thence till the 14th of September; and in deep water, Eaſt of Yarmouth, and as far as the mouth of the Thames, from thence to the 25th of November, when the regular fiſhery ceaſes. But herrings are found not far from Yarmouth till the end of January, after which the fiſhery is prohibited, as the ſpawning ſeaſon then commences.

(To be continued.)

Air Pump
by
Mr. Mendelsjohn



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Mr. Gough's
Theory of the
Speaking Trumpet.

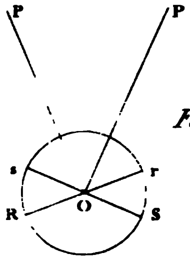

Fig. 4.

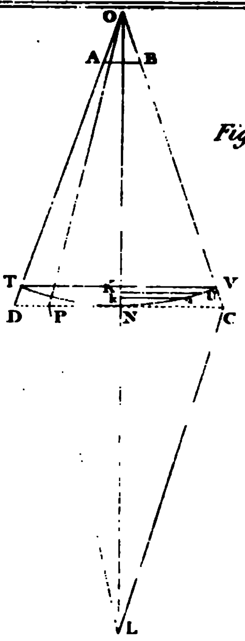

Fig. 3.

Part of the Air Pump.

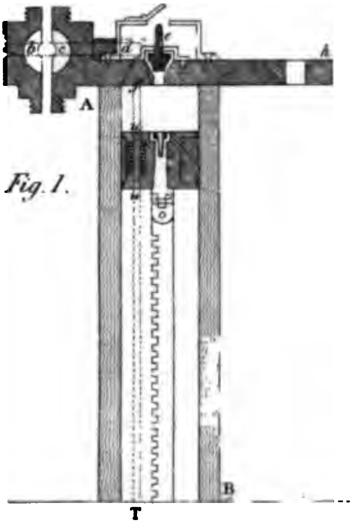

Fig. 1.

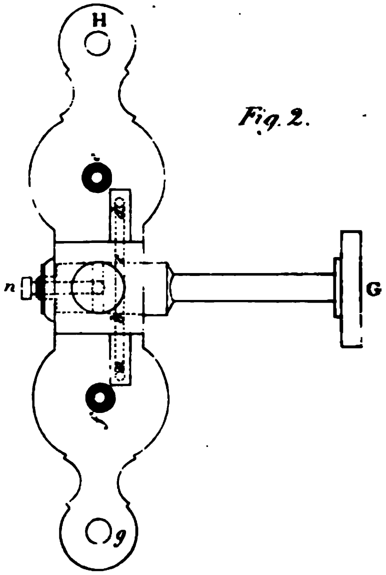

Fig. 2.

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Fig. 1.

Improvement in Woulfe's Apparatus.

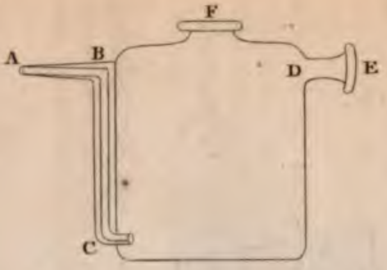


Fig. 3.

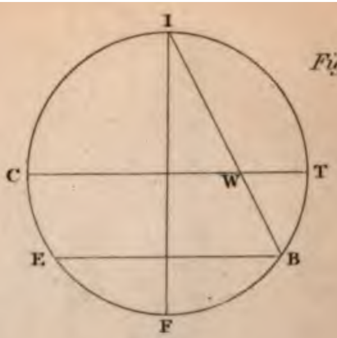
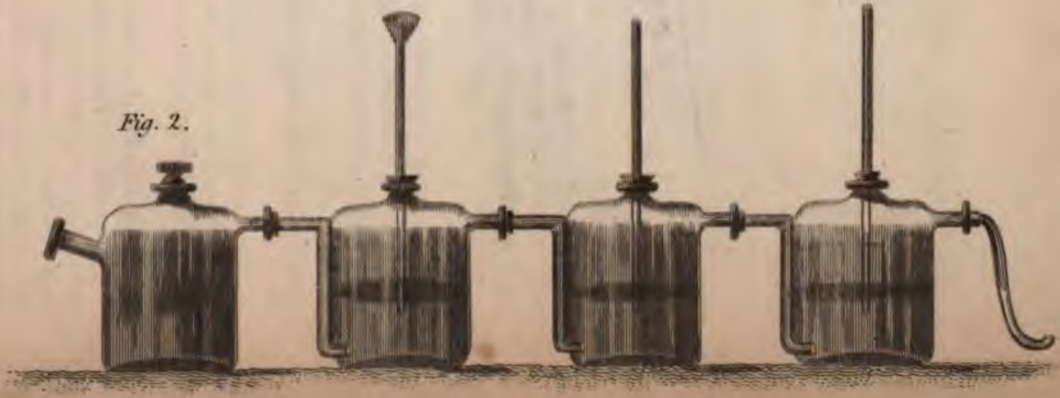


Fig. 2.



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A
JOURNAL
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NATURAL PHILOSOPHY, CHEMISTRY,
AND
THE ARTS.

APRIL, 1805.

ARTICLE I.

A ſhort Account of the Cauſe of the Diſeaſe in Corn, called by Farmers, the Blight, the Mildew and the Ruſt. Reprinted, and the Plates copied, with Permiſſion, from a Memoir communicated by the Right Hon. SIR JOSEPH BANKS, *Bart. P. R. S. &c. &c. &c. With ſome additional Notes.**

BOTANISTS have long known that the Blight in Corn is occaſioned by the growth of a minute paraſitic fungus or muſhroom on the leaves, ſtems, and glumes of the living plant.

Botaniſts have long known that the blight is cauſed by a minute paraſitic fungus;

Felice

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* Immediately after the title in the original is the following prefatory note:

" The following brief Publication, ſuggeſted by the alarming " ſtate of the harveſt in Auguſt laſt, would have been diſtributed " before the end of wheat ſeed-time, had the engraver fulfilled his " engagement.

" This circumſtance will, it is hoped, be conſidered as a ſufficient " apology for the want of actual obſervations on the origin and pro" greſs of the diſeaſe. Theſe it is preſumed will be abundantly " ſupplied in the courſe of the preſent year, by thoſe intelligent " agriculturiſts, whoſe reſidence in the country enables them daily " to examine, not only the progreſs of their crops, but the origin " and advances alſo of all thoſe obſtacles which nature has oppoſed to the ſucceſs of agricultural labours, as if to awaken the ener-

Felice Fontana publiſhed in the year 1767 an elaborate account of this miſchievous weed,* with microſcopic figures, which give a tolerable idea of its form; more modern botaniſts † have given figures both of corn and of graſs affected by it, but have not uſed high magnifying powers in their reſearches.

but agriculturiſts have paid little attention to this fact.

Agriculturiſts do not appear to have paid, on this head, ſufficient attention to the diſcoveries of their fellow-labourers in the field of nature; for though ſcarce any Engliſh writer of note on the ſubject of rural economy has failed to ſtate his opinion of the origin of this evil, no one of them has yet attributed it to the real cauſe, unleſs Mr. Kirby's excellent papers on ſome diſeaſes of corn, publiſhed in the Tranſactions of the Linnæan Society, are conſidered as agricultural eſſays.

Engravings of this deſtructive plant from accurate drawings by Mr. Bauer are here offered to their conſideration.

On this account it has been deemed expedient to offer to the conſideration of farmers, engravings of this deſtructive plant, made from the drawings of the accurate and ingenious Mr. Bauer, Botanical Painter to his Majeſty, accompanied with his explanation, from whence it is preſumed an attentive reader will be able to form a correct idea of the facts intended to be repreſented, and a juſt opinion whether or not they are, as is preſumed to be the caſe, correct and ſatisfactory.

Organized ſtructure of the ſurface of ſtraw. Its pores or mouths, ſhut in dry and open in wet weather.

In order, however, to render Mr. Bauer's explanation more eaſy to be underſtood, it is neceſſary to premiſe, that the ſtriped appearance of the ſurface of a ſtraw which may be ſeen with a common magnifying glaſs, is cauſed by alternate longitudinal partitions of the bark, the one imperforate, and the other furniſhed with one or two rows of pores or mouths, ſhut in dry, open in wet weather, and well calculated to imbibe fluid whenever the ſtraw is damp. ‡

By

" gies of reaſon, and to reward the farmer for the exertions of his " intellectual faculties, by the ſatisfaction of ſurmounting them."

" Jan. 30th, 1805." " JOS. BANKS."

* Oſſervazioni ſopra la Ruggine del Grano. Lucca, 1767, 8vo.

† Sowerby's Engliſh Fungi, Vol. II. Tab. 140, Wheat Tab. 139. Poa aquatica.

The pores or mouths on the ſurface of plants are deſigned to abſorb moiſture.

‡ Pores or mouths ſimilar to theſe are placed by nature on the ſurface of the leaves, branches, and ſtems, of all perfect plants, a proviſion intended no doubt to compenſate, in ſome meaſure, the want of loco-motion in vegetables. A plant cannot when thirſty go

By theſe pores, which exiſt alſo on the leaves and glumes, it is preſumed that the ſeeds of the fungus gain admiſſion, and at the bottom of the hollows to which they lead, (ſee Plate IX. Fig. 1, 2,) they germinate and puſh their minute roots, no doubt (though theſe have not yet been traced) into the cellular texture beyond the bark, where they draw their nouriſhment, by intercepting the ſap that was intended by nature for the nutriment of the grain; the corn of courſe becomes ſhrivelled in proportion as the fungi are more or leſs numerous on the plant; and as the kernel only is abſtracted from the grain, while the cortical part remains undiminiſhed, the proportion of *flour to bran* in blighted corn, is aways reduced in the ſame degree as the corn is made light. Some corn of this year's crop will not yield a ſtone of flour from a ſack of wheat; and it is not impoſſible that in ſome caſes the corn has been ſo completely robbed of its flour by the fungus, that if the proprietor ſhould chooſe to incur the expenſe of thraſhing and grinding it, bran would be the produce, with ſcarce an atom of flour for each grain.

The ſeeds of the fungus enter the pores,

germinate in the cellular texture, intercept the ſap, and render the grain ſhrivelled by abſtraction of the kernel.

The flour may thus be almoſt totally intercepted.

Every ſpecies of corn, properly ſo called, is ſubject to the blight; but it is obſervable that ſpring corn is leſs damaged by it than winter, and rye leſs than wheat, probably becauſe it is ripe and cut down before the fungus has had time to increaſe in any large degree.—Tull ſays that "white cone or "bearded wheat, which hath its ſtraw like a ruſh full of pith, "is leſs ſubject to blight than Lammas wheat, which ripens a "week later." See page 74. The ſpring wheat of Lincolnſhire was not in the leaſt ſhrivelled this year, though the ſtraw was in ſome degree infected: the millers allowed that it was the beſt ſample brought to market. Barley was in ſome places conſiderably ſpotted, but as the whole of the ſtem of that grain is naturally enveloped in the hoſe or baſis of the leaf, the fungus can in no caſe gain admittance to the ſtraw; it is how-

All corn is liable to the blight; ſpring corn leſs than winter, and rye leſs than wheat.

Hiſtorical facts.

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go to the brook and drink, but it can open innumerable orifices for the reception of every degree of moiſture, which either falls in the ſhape of rain and of dew, or is ſeparated from the maſs of water always held in ſolution by the atmoſphere; it ſeldom happens in the drieſt ſeaſon, that the night does not afford ſome refreſhment of this kind, to reſtore the moiſture that has been exhauſted by the heats of the preceding day.

ever to be obſerved that barley riſes from the flail lighter this year than was expected from the appearance of the crop when gathered in.

Little information was obtained by enquiry reſpecting blight; but its cauſe being now explained, the remedy it is hoped will be diſcovered.

Though diligent enquiry was made during the laſt autumn, no information of importance relative to the origin or the progreſs of the blight could be obtained: this is not to be wondered at; for as no one of the perſons applied to had any knowledge of the real cauſe of the malady, none of them could direct their curioſity in a proper channel. Now that its nature and cauſe have been explained, we may reaſonably expect that a few years will produce an intereſting collection of facts and obſervations, and we may hope that ſome progreſs will be made towards the very deſirable attainment of either a preventive or a cure.

Progreſs of the infection.

It ſeems probable that the leaf is firſt infected in the ſpring, or early in the ſummer, before the corn ſhoots up into ſtraw, and that the fungus is then of an orange colour;* after the ſtraw has become yellow, the fungus aſſumes a deep chocolate brown: each individual is ſo ſmall that every pore on a ſtraw will produce from 20 to 40 fungi, as may be ſeen in the plates, and every one of theſe will no doubt produce at leaſt 100 ſeeds; if then one of theſe ſeeds tillows out into the number of plants that appear at the bottom of a pore in *Plate* IX. *Fig*. 1, 2. how incalculably large muſt the increaſe be! A few diſeaſed plants ſcattered over a field muſt very ſpeedily infect a whole neighbourhood, for the ſeeds of fungi are not much heavier than air, as every one who has trod upon a ripe puff-ball muſt have obſerved by ſeeing the duſt, among which is its ſeed, riſe up and float on before him.

Increaſe of the fungus incalculably rapid;

and its periods of generation very quick.

How long it is before this fungus arrives at puberty, and ſcatters its ſeeds in the wind, can only be gueſſed at by the analogy of others; probably the period of a generation is ſhort, poſſibly not more than a week in a hot ſeaſon: if ſo, how frequently in the latter end of the ſummer muſt the air be loaded as it were with this animated duſt, ready, whenever a gentle breeze, accompanied with humidity, ſhall give the ſignal to intrude itſelf into the pores of thouſands of acres of corn. Providence, however, careful of the creatures it has created, has benevolently provided againſt the too extenſive multiplication of any ſpecies of being; was it otherwiſe, the minute plants and animals, enemies againſt which man has the feweſt means of defence, would increaſe to an inordinate extent; this however, can in no caſe happen, unleſs many prediſpoſing cauſes afford their combined aſſiſtance. But for this wiſe and beneficent proviſion, the plague of ſlugs, the plague of mice, the plagues of grubs, wire-worms, chafers, and many other creatures whoſe power of multiplying is countleſs as the ſands of the ſea, would, long before this time, have driven mankind, and all the larger animals, from the face of the earth.

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* The Abbé Teſſier, in his *Traité des Maladies des Grains*, tells us that, in France, this diſeaſe firſt ſhews itſelf in minute ſpots of a dirty white colour on the leaves and ſtems, which ſpots extend themſelves by degrees, and in time change to yellow, and throw off a dry orange coloured powder. pp. 201, 340. *Note of Sir J. B*

If there were no proviſion in nature to check the increaſe of ſmall plants and animals, the larger would ſpeedily be deſtroyed by them.

Though all old perſons who have concerned themſelves in agriculture remember the blight in corn many years, yet ſome have ſuppoſed that of late years it has materially increaſed; this however does not ſeem to be the caſe. Tull, in his Horſe-hoeing Huſbandry, p. 74, tells us, that the year 1725 "was a "year of blight the like of which was never before heard of, and "which he hopes may never happen again;" yet the average price of wheat in the year 1726, when the harveſt of 1725 was at market, was only 36*s.* 4*d.* and the average of the five years of which it makes the firſt, 37*s.* 7*d.*—1797 was alſo a year of great blight; the price of wheat in 1798 was 49*s.* 1*d.* and the average of the five years, from 1795 to 1799, 63*s.* 5*d.*[*]

The blight does not appear to have increaſed of late years.

The climate of the Britiſh Iſles is not the only one that is liable to the blight in corn; it happens occaſionally in every part of Europe, and probably in all countries where corn is grown.

The blight in corn probably happens in all countries.

[*] The ſcarcity of the year 1801 was in part occaſioned by a mildew which, in many places, attacked the plants of wheat on the S.E. ſide only: but it was principally owing to the very wet harveſt of 1800. The deficiency of wheat, at that harveſt, was found, on a very accurate calculation, ſomewhat to exceed one fourth. But wheat was not the only grain that failed: all others, and potatoes alſo were materially deficient. This year the wheat is probably ſomewhat more damaged than it was in 1800, and barley ſomewhat leſs than an average crop. Every other article of agricultural food is abundant, and potatoes one of the largeſt crops that has been known. But for theſe bleſſings on the labour of man, wheat muſt before this time have reached an exorbitant price.—*Sir J. B.*

Italy is very ſubject to it; and the laſt harveſt of Sicily has been materially hurt by it. Specimens received from the colony of New South Wales ſhew that conſiderable miſchief was done to the wheat crop there in the year 1803 by a paraſitic plant, very ſimilar to the Engliſh one.

Common opinion that wheat in the neighbourhood of barberries is moſt frequently blighted.

It has been long admitted by farmers, though ſcarcely credited by botaniſts, that wheat in the neighbourhood of a barberry buſh ſeldom eſcapes the blight. The village of Rolleſby in Norfolk, where barberries abound, and wheat ſeldom ſucceeds, is called by the opprobrious appellation of Mildew Rolleſby. Some obſerving men have of late attributed this very perplexing effect to the farina of the flowers of the barberry, which is in truth yellow, and reſembles in ſome degree the appearance of the ruſt, or what is preſumed to be the blight in its early ſtate.

The barberry leaf is very ſubject to a mildew fungus;

It is, however, notorious to all botanical obſervers, that the leaves of the barberry are very ſubject to the attack of a yellow paraſitic fungus, larger, but otherwiſe much reſembling the ruſt in corn.

which may be transferred to corn.

Is it not more than poſſible that the paraſitic fungus of the barberry and that of wheat are one and the ſame ſpecies, and that the ſeed transferred from the barberry to the corn is one cauſe of the diſeaſe. Miſletoe, the paraſitic plant with which we are the beſt acquainted, delights moſt to grow on the apple and hawthorn, but it flouriſhes occaſionally on trees widely differing in their nature from both of theſe: in the Home Park, at Windſor, miſletoe may be ſeen in abundance on the lime trees planted there in avenues. If this conjecture is founded, another year will not paſs without its being confirmed by the obſervations of inquiſitive and ſagacious farmers.

Conjectures directed to remedy.

It would be preſumptuous to offer any remedy for a malady, the progreſs of which is ſo little underſtood; conjectures, however, founded on the origin here aſſigned to it, may be hazarded without offence.

Probable fact as to its earlieſt appearance.

It is believed * to begin early in the ſpring, and firſt to appear on the leaves of wheat in the form of ruſt, or orange-coloured powder; at this ſeaſon, the fungus will, in all probability,

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* This, though believed, is not dogmatically aſſerted, becauſe Fontana, the beſt writer on the ſubject, aſſerts that the yellow and the dark-coloured blight are different ſpecies of fungi.

bability, require as many weeks for its progreſs from infancy to puberty as it does days during the heats of autumn; but a very few plants of wheat, thus infected, are quite ſufficient if the fungus is permitted to ripen its ſeed, to ſpread the malady over a field, or indeed over a whole pariſh.

The chocolate coloured blight ſeems to ſpread from central points of the field.

The chocolate-coloured blight is little obſerved till the corn is approaching very nearly to ripeneſs; it appears then in the field in ſpots, which increaſe very rapidly in ſize, and are in calm weather ſomewhat circular, as if the diſeaſe took its origin from a central poſition.

Whence it may have been brought in by infected ſtraw in the manure;

May it not happen then, that the fungus is brought into the field in a few ſtalks of infected ſtraw uncorrupted among the maſs of dung laid in the ground at the time of ſowing? it muſt be confeſſed, however, that the clover lays, on which no dung from the yard was uſed, were as much infected laſt autumn as the manured crops. The immenſe multiplication of the diſeaſe in the laſt ſeaſon, ſeems however to account for this; as the air was no doubt frequently charged with ſeed for miles together, and depoſited it indiſcriminately on all ſorts of crops.

and may, it is likely, be guarded againſt by extirpating the plants firſt infected.

It cannot however be an expenſive precaution to ſearch diligently in ſpring for young plants of wheat infected with the diſeaſe, and carefully to extirpate them, as well as all graſſes, for ſeveral are ſubject to this or a ſimilar malady, which have the appearance of orange-coloured or of black ſtripes on their leaves, or or their ſtraw; and if experience ſhall prove that uncorrupted ſtraw can carry the diſeaſe with it into the field, it will coſt the farmer but little precaution to prevent any mixture of freſh ſtraw from being carried out with his rotten dung to the wheat field.

Whether blighted ſtraw be more or leſs nutritious than clean ſtraw, for cattle?

In a year like the preſent, that offers ſo fair an opportunity, it will be uſeful to obſerve attentively whether cattle in the ſtraw yard thrive better or worſe on blighted than on healthy ſtraw. That blighted ſtraw, retaining on it the fungi that have robbed the corn of its flour, has in it more nutritious matter than clean ſtraw which has yielded a crop of plump grain, cannot be doubted; the queſtion is, whether this nutriment in the form of fungi does, or can be made to agree as well with the ſtomachs of the animals that conſume it, as it would do in that of ſtraw and corn.

It

Though the ſeeds of blighted corn afford little flour, they are each as good for ſowing as the plumpeſt ſamples; and are much more numerous in an equal meaſure.

It cannot be improper in this place to remark, that although the ſeeds of wheat are rendered, by the exhauſting power of the fungus, ſo lean and ſhrivelled that ſcarce any flour fit for the manufacture of bread can be obtained by grinding them, theſe very ſeeds will, except, perhaps, in the very worſt caſes *, anſwer the purpoſe of ſeed corn as well as the faireſt and plumpeſt ſample that can be obtained, and, in ſome reſpects better; for as a buſhel of much blighted corn will contain one-third at leaſt more grains in number than a buſhel of plump corn, three buſhels of ſuch corn will go as far in ſowing land, as four buſhels of large grain.

The flour in the ſhrivelled grain is amply ſufficient to nouriſh the minute plants till they take root.

The uſe of the flour of corn in furthering the proceſs of vegetation, is to nouriſh the minute plant from the time of its developement till its roots are able to attract food from the manured earth; for this purpoſe one-tenth of the contents of a grain of good wheat is more than ſufficient. The quantity of flour in wheat has been increaſed by culture and management calculated to improve its qualities for the benefit of mankind, in the ſame proportion as the pulp of apples and pears has been increaſed by the ſame means, above what is found on the wildings and crabs in the hedges.

It is a waſteful practice to take the plumpeſt grain for ſeed. The ſmalleſt though unfit for the mill are equally good for this uſe.

It is cuſtomary to ſet aſide or to purchaſe for ſeed corn, the boldeſt and plumpeſt ſamples that can be obtained; that is, thoſe that contain the moſt flour; but this is unneceſſary waſte of human ſubſiſtence; the ſmalleſt grains, ſuch as are ſifted out before the wheat is carried to market, and either conſumed in the farmer's family, or given to his poultry, will be found by experience to anſwer the purpoſe of propagating the ſort from whence they ſprung, as effectually as the largeſt.

Arrangement of the grains in an ear of wheat. The lower ones ſupply the moſt food, but the upper are not inferior as ſeed.

Every ear of wheat is compoſed of a number of cups placed alternately on each ſide of the ſtraw; the lower ones contain, according to circumſtances, three or four grains, nearly equal in ſize, but towards the top of the ear, where the quantity of nutriment is diminiſhed by the more ample ſupply of thoſe cups that are nearer the root, the third or fourth grain in a cup is frequently defrauded of its proportion, and becomes

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* 80 grains of the moſt blighted wheat of the laſt year, that could be obtained, were ſown in pots in the hot-houſe; of theſe, ſeventy-two produced healthy plants, a loſs of 10 per cent. only.

ſhrivelled and ſmall. Theſe ſmall grains, which are rejected by the miller, becauſe they do not contain flour enough for his purpoſe, have nevertheleſs an ample abundance for all purpoſes of vegetation, and as fully partake of the ſap, (or blood, as we ſhould call it in animals,) of the kind which produced them, as the faireſt and fulleſt grain that can be obtained from the bottoms of the lower cups by the waſteful proceſs of beating the ſheaves.

EXPLANATION OF THE PLATES.

Plate IX.

Fig. 1. A piece of the infected wheat ſtraw—natural ſize: at *a* the leaf ſheath is broken and removed, to ſhew the ſtraw which is not infected under it.

Fig. 2. A highly magnified repreſentation of the paraſitic plant which infects the wheat: *a* in a young ſtate; *b* full grown; *c* are two plants burſting and ſhedding their ſeeds when under water in the microſcope; *d* two plants burſt in a dry ſtate; *e* ſeems to be abortive; *f* ſeeds in a dry ſtate; *g* a ſmall part of the bottom of a pore with ſome of the paraſitic fungi growing upon it.

Fig. 3. A part of the ſtraw of *fig.* 1, magnified.

Fig. 4. Part of *fig.* 3 at *a b* more magnified.

Fig. 5. Part of a ſtraw ſimilar to *fig.* 3, but in its green ſtate, and before the paraſitic plant is quite ripe.

Fig. 6. A ſmall part of the ſame, more magnified.

Plate X.

Fig. 1. A highly magnified tranſverſe cutting of the ſtraw, correſponding with *Fig.* 4. *Plate* I. ſhewing the inſertion of the paraſite in the bark of the ſtraw.

Fig. 2. A longitudinal cutting of the ſame; magnified to the ſame degree.

Fig. 3. A ſmall piece of the epidermis of a ſtraw, ſhewing the large pores which receive the ſeed of the paraſite; the ſmaller ſpots obſervable on the epidermis, are the baſes of hairs that grow on the plant of the wheat whilſt young, but which fall off when it ripens, magnified to the ſame degree as the preceding figures.

II.

Theorems reſpecting the Properties of the Sides of Triangles interſected by Right Lines drawn from the three Angles ſo as to meet in one Point. By Mr. JOHN GOUGH.

To Mr. NICHOLSON.

SIR,

Middleſhaw, March 1, 1805.

Certain properties of triangles.

GEOMETRICIANS have paid but little attention to the properties of the ſides of triangles, which are interſected by right lines, drawn from the three oppoſite angles ſo as to meet in one point. Perhaps all the theorems of this kind that have been hitherto publiſhed, are contained in the ſecond book of Emerſon's Geometry, with the ſingle exception of a propoſition given by Mr. Landen; who has proved that if one of the internal angles of a tringle, and the two oppoſite external angles of the ſame be biſected, the right lines, drawn for this purpoſe, will alſo meet in a common point. The following propoſitions take a more general view of the ſubject; and as ſome mathematical papers have appeared of late in the Philoſophical Journal, I have ventured to aſk a place for the preſent ſheet, in your valuable miſcellany.

I remain, &c.
JOHN GOUGH.

P. S. In vol. X. page 66, line 2, for 5½ yards read 3½ yards.

Propoſition 1ſt. Let three right lines C M, B N and A S, (*Plate* XI. *Fig.* 1, 2.) drawn from the three angles of a triangle A B C, interſect the oppoſite ſides, produced or not, in the points M, N and S, and alſo meet each other in a common point O; then if all the points M, N and S lay betwixt the angular points B, A and C, the place of O will be within the triangle, and the three lines C O, B O and A O will fall wholly within the ſame; but if one of theſe points, as S, lay in B E, which is C B produced, the place of O will be external to the triangle; in which caſe, one of the two remaining points of interſection will lay in its reſpective ſide produced; but the ſituation of the other will always be betwixt the angular points of the third ſide.

Certain properties of triangles.

Demonſtration.

Caſe 1*ſt*. Let M, N and S (*Fig.* 1.) be betwixt the angular points B, A and C; in which caſe, the triangles A B C, M B C are of the ſame altitude; conſequently they are as their baſes A B, B M, *Euc.* 1.6; but B M is a part of A B by hypotheſis; therefore the triangle M B C is a part of A B C, (*Simpſon's Euclid.* D. 5.) that is, the triangle A B C contains the triangle M B C; for the ſame reaſon, the ſame A B C contains the triangle S A B; but the figure M B S O is common to the triangles M B C, S A B; therefore it is contained in the triangle A B C; conſequently the place of O is within the ſame: now as the points A, B, C and O are all found in the ſpace A B C, the lines A O, B O and C O muſt alſo lay wholly within the ſame triangle.

Caſe 2. When the point S falls in B E or C B produced, (*Fig.* 2.) the angle C B A is external to the triangle A B S; therefore it is greater than the angle B S A, (*Euc.* 17, 1;) and the ſum of the angles C A B, A C B is leſs than the angles C A S, A C S taken together, (*Euc.* 32. 1); hence the angle C A B. is leſs than C A S; conſequently the right line A S, produced at pleaſure, falls wholly without the triangle A B C. Now if the remaining two lines C M and B N be ſuppoſed to cut the ſides B A and A C not produced, the place of O, their common interſection, will be in the ſpace A B C, by *Caſe* 1: but this is impoſſible; for O is in the right line A S by hypotheſis; which has been ſhewn to fall wholly without the triangle A B C, conſequently one at leaſt of the lines C M, B N muſt alſo lay without the ſame ſpace; let this be B N, and draw O K parallel to C A; then the external angle O K E of the triangle O K C is greater than the internal and oppoſite angle

Certain properties of triangles.

angle B C M, 17.1; but the angles O K E, A C B are equal (*Euc.* 29.1;) therefore the triangle M B C is a part of the triangle A B C, and is contained in it; but theſe triangles are as their baſes M B, B A (*Euc.* 1.6;) conſequently M B is a part of B A, Simſ. *Euc.* D. 5; and the point M is between the points A and B. Q. E. D.

Corollary. Hence it appears that if two lines C M and B N be drawn, and their interſection O lay within the triangle A B C, the place of the third point S will be in the ſide B C unproduced. But if O lay without the triangle, the place of the third point will depend on the ſituation of the given points; namely if theſe be in two ſides produced, the place ſought will lay in the remaining ſide unproduced; but if either of the given points fall between two angles of the triangle, the third will be found in the remaining ſide produced.

Propoſition 2. If a right line C M (*Fig.* 1.) be drawn from C, one of the angles of a triangle A B C, to cut the oppoſite ſide A B, produced or not, in M, and if from the point M, perpendiculars, M F, M H be let fall upon the ſides A C, C B, the rectangle B M, M F will be to the rectangle A M, M H, as B C is to C A.

Demonſtration. Draw A P, B Q perpendicular to B C, C A; then we have the following proportions, by ſemilar triangles; as M B : M H :: A B : A P; and as M F : M A :: B Q : B A; hence as M B × M F : A M × M H :: B Q : A P. But as B Q : A P :: B C : C A, *Euc.* 14.6; therefore the rectangle M B, M F is to the rectangle M A, M H as B C is to C A. *Euc.* 11.5. Q. E. D.

Cor. 1*ſt.* If O *h*, O *f* be drawn perpendicular to B C, C A, from any point O in M C; the rectangle M B, O *f* will be to M A, O *h* as B C is to C A; for by ſimilar triangles as *f* O : O *h* :: F M : M H; hence as M B × O *f* : M A × O *h* :: M B × M F : M A × M H; conſequently as M B × O *f* : M A × O *h* :: B C : C A. *Euc.* 11.5.

Cor. 2*nd.* Let the triangle A B C be iſoſceles, having the ſides B A, A C equal; alſo let C M, B N, drawn to the oppoſite ſides B A, A C meet in O; then the point S is given, in which A O, produced if neceſſary, cuts the remaining ſide C B. For draw O *f*, O *g* perpendicular to C A, A B, and we ſhall have, as B S × O *f* : C S × O *g* :: B A : A C, *Cor.* 1; but

B A, A C are equal by hypotheſis; therefore $BS \times Of = CS \times Og$; hence as $Og : Of :: BS : SC$; conſequently the point S may be found, proper attention being paid to *Cor. Prop.* 1.

Certain properties of triangles.

Propoſition 3. If three right lines C M, B N, A S, drawn from the three angles of a triangle A B C (*Fig.* 1 and 2) meet each other in a point O, and the oppoſite ſides B A; A C and C B, in M, N and S; the ſegments of any one ſide are as the rectangle under the alternate ſegments of the remaining two ſides; *i.e.* B S is to S C as the rectangle B M, A N to the rectangle C N, A M.

Demonſtration. Draw Of, Og, Oh perpendicular to C A, A B and B C. Then as $BC : CA :: BM \times Of : AM \times Oh$; *Cor.* 1. *Prop.* 2; for the ſame reaſon as $AB : BC :: NA \times Oh : NC \times Og$; hence as $BA : AC :: MB \times NA \times Of : NC \times MA \times Og$; but as $BA : AC :: BS \times Of : CS \times Og$; conſequently B S is to S C as the rectangle B M, A N is to the rectangle C N, A M. Q. E. D.

Cor. If two right lines C M, B N be drawn from two angles of any plain triangle A B C, ſo as to meet each other in a point O, and the oppoſite ſides B A, A C in M and N; a point S may be found in the remaining ſide, produced if neceſſary, ſo that if S A be joined, the right line A S ſhall paſs through O. For ſince the right lines B M, M A, A N and N C are given, the rectangles B M, A N and C N, A M are alſo given; conſequently the ratio of theſe rectangles is given: but the right line A O cuts the ſide C B, produced or not, according to circumſtances, in the ſame ratio, by the laſt propoſition; therefore if both M and N lay betwixt the angular points of the triangle, or in their reſpective ſides produced, divide B C in S, ſo that B S may be to S C in the ratio of the rectangles B M, A N and C N, A M; then will S be the required point by *Cor.* to *Prop.* 1; but if M be ſituated betwixt the angles B, A, and N lay in C A produced; find S in C B alſo produced, ſo that B S may be to S C as the rectangle B M, A N is to the rectangle C N, A M; and S will be the required point, by the corollary laſt referred to.

Prop. 4. Let three right lines C M, B N and A S (*Fig.* 1 and 2) drawn from the three angles of a triangle A B C, interſect each other in a common point O, and meet the oppoſite

Certain properties of triangles.

ſite ſides B A, A C and C B, in the points M, N and S; if A N be to N C as A^pB is to B^pC; but B M be to M A as B^rC is to C^rA; then will B S be to S C as the product of B^pA into B^rC is to the product of B^pC into A^rM.

Demonſtration. As A N : N C :: B^pA : B^pC; and as B M : M A :: B^rC : C^rA by hypotheſis; hence as B M × A N : C N × A M :: B^pA × B^rC : B^pC × C^rA; but as B M × A N : N C × A M :: B S : S C; conſequently B S is to S C as the product of B^pA into B^rC is to the product of B^pC into C^rA. *Euc.* 11. 5. Q. E. D.

Corollary 1ſt. The preceding theorem is general; for the ratios of B M to M A, and of A N to N C can be expreſſed by ſome power or powers of the lines B C, C A and A B, B C, which contain the angles oppoſite to the ſides B A and A C; therefore if the points M, N and S be arranged according to the corollary to Propoſition 1ſt, and the ſegments betwixt theſe points and the adjacent angles have the ratios aſſigned in the preſent theorem, right lines, drawn from M, N and S to the oppoſite angles, will all meet in one point.

Cor. 2. When r and p are equal, B S is to S C as B^pA is to A^pC by the propoſition; therefore if the ſegments betwixt the points M, N, S and the adjacent angles are in the ratios of the adjacent ſides raiſed to any given power, right lines, drawn from M, N and S to the oppoſite angles, will all meet in one point by *Cor. 1ſt.*

Cor. 3rd. Let $p = o$; then as B S : S C :: B^oA : A^oC by the propoſition; but B^oA : A^oC is the ratio of equality; therefore if M, N and S biſect the three ſides of a triangle, lines drawn from them to the oppoſite angles will meet in one point by *Cor. 2nd.*

Cor. 4. Let $p = 1$; alſo let M and N interſect the ſides B A, A C not produced; then S lays between the angular points B, C, *Cor. Prop.* 1; and as B S : S C :: B A : A C, *Cor.* 2; therefore the right line S A biſects the angle B A C; conſequently if three right lines biſect the three internal angles of a triangle, they ſhall all meet in one point by the laſt propoſition.

Cor. 5. Let $p = 1$ as before; alſo ſuppoſe M to be in the ſide B A not produced; but let N be without the triangle; then S is in C B produced, *Cor. Prop.* 1; but as B S : S C ::

B A : A C, *Cor.* 2; therefore S A biſects the angle B A N. (*Simpſon's Euclid.* .6 A); alſo as B M : M A : : B C : C A; therefore C M biſects the angle A C B, *Euc.* 3.6; conſequently if two lines biſect any two of the external angles of triangles, and a third line biſect the internal angle oppoſite to theſe angles, theſe lines ſhall meet in one point by the laſt propoſition.

Prop. 5. If B M be to M A as $B^{p}C$ to $C^{p}A$; and hO, Of be perpendicular to B C and C A; hO is to Of as $B^{p-1}C$ to $C^{p-1}A$.

Demonſtration. As $BM \times Of : AM \times Oh :: BC : CA$, *Cor.* 1. *Prop.* 2; and as $B^{p}C : A^{p}C :: BM : MA$ by hypotheſis; therefore as $B^{p}C \times Of : A^{p}C \times Oh :: BC : CA$; conſequently as $Oh : Of :: B^{p-1}C : C^{p\;1}A$. Q. E. D.

III.

A Communication on the Uſe of Green Vitriol, or Sulphate of Iron, as a Manure; and on the Efficacy of paring and burning depending, partly, on Oxide of Iron. By GEORGE PEARSON, M. D. *Honorary Member of the Board of Agriculture, F. R. S. From a Communication made by him to the Board, and inſerted in the fourth Volume of their Tranſactions.*

(Concluded from page 214.)

APPENDIX.

THE following facts, lately diſcovered by moſt reſpectable chemiſts, appear to be worth adding to the preceding memoir, as they ſerve to ſhew that other ſalts, beſides ſulphate of iron and certain earths, may be employed advantageouſly as manures, although like iron they have been eſteemed deleterious to plants.

1. *Aſhes of Pit Coal are a good Manure for Graſs.*

The aſhes of pit coal are a good manure for graſs.

My much valued friend, the Rev. William Gregor, of Grampound, on examination of the aſhes of coal from Liverpool, found them to contain both ſulphate of magneſia and ſulphate of lime, eſpecially the former, ſalt. I apprehend that theſe aſhes alſo contain oxide of iron, or perhaps ſulphate of iron. These aſhes, ſays Mr. Gregor, "*ſcaded*" * over graſs apparently produced good effects *notwithſtanding the ſulphate of magneſia*, which I was well aſſured they contained. See Nicholſon's Journal, Vol. V. p. 225.

Sulphate of magneſia may not be hurtful to plants, tho' pure magneſia is.

From this obſervation of Mr. Gregor, it ſeems he is aware of the prevailing popular opinion, that ſulphate of magneſia is not favourable to vegetation; and to reconcile his fact with the unfriendly nature of magneſia to plants, as diſcovered by Mr. Tennant, he obſerves that the effects of ſulphate of magneſia may be very different from thoſe of magneſia and carbonate of magneſia. I apprehend it is the magneſia (calcined magneſia) only which this learned chemiſt found hurtful to vegetables, as the diſcovery was made on the examination of Nottingly lime, which the farmers near Doncaſter employ as a manure, while they reject the lime of their own neighbourhood. In the latter Mr. Tennant met with magneſia, and in the former none. See the account of this important diſcovery in the Philoſ. Tranſactions.

2. *The Earth from Aſhes called Cinis, is a durable and efficacious Manure; by Profeſſor Mitchill, of New York, one of the Repreſentatives in Congreſs. Addreſſed to Dr. Pearſon.*

Earth from aſhes is a good manure.

Dr. Mitchill, in a letter addreſſed to me on *cinis*, or earth found in the aſhes of wood, has made ſome obſervations relative to the preceding memoir, which ſeem worthy of notice.

" Aſhes of wood contain very commonly ſulphate of potaſh, alſo phoſphoric acid, beſides other well known ſalts; but after theſe ſalts are ſeparated by lixiviation, there remains a *peculiar earth*, and a ſmall proportion of iron. This earth differs from lime, baryt, magneſia, ſtrontian, or any other known ſpecies of earth. I would call it *cinis*, for plentiful, common, and important as it is, ſcience has not dignified it with a name. To judge of the excellence of this earth as a manure, after all the ſalts are extracted from ſoap boilers' aſhes, the earth ſells for ten cents the buſhel, and notwithſtanding this high price, it is not unuſual for the farmer to pay for the article twelve months beforehand. When ploughed into ſterile ground, at twelve loads per acre, it produces great crops of wheat, clover, and other ſorts of graſs and grain, and its fertilizing operation will laſt twenty years. Although ſome of the other ingredients of the aſhes left after lixiviation may prove beneficial, yet the effects are chiefly from the cinis, or new named earth.

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* From Σκεδαω.

"This earth, which is ſo prized in America as a manure, was eſteemed of old in Aſia, as an ingredient in a cement: among the ancient Syrians, it was one of the materials forming the plaſter of their walls; and as it holds an intermediate place between lime and potaſh, it can eaſily be conceived how it may act both as a cement and a manure. It is to be hoped, chemiſts will turn their attention to this important ſubject." See Tilloch's Philoſ. Magazine, Vol. VII. p. 273, for the whole of this intereſting letter.

3. *Several Metallic Salts promote Vegetation, ſhewn by the Experiments of Profeſſor Barton, of Philadelphia.*

Letter from Benjamin Smith Barton, M. D. Profeſſor of Medicine in the College of Philadelphia, to Dr. Pearſon, containing Experiments with Metallic Solutions to determine their Effects on Plants.

SIR,

Philadelphia, Oct. 28, 1802.

Direct experiments on the effect of metallic ſalts on vegetation.

IN the *Annals of Medicine* for the year 1801, you inform us, that you have lately read a paper at the Board of Agriculture, "containing an account of the effects of a ſaline body collected from peat, as a moſt powerful manure, which turns out to be ſulphate of iron; a ſubſtance (you remark) hitherto conſidered to be a poiſon to plants." This piece of intelligence gave me much ſatisfaction. I have, for ſome years, been engaged in an extenſive ſeries of experiments, relative to the effects of various ſtimulating articles, ſuch as camphire, &c. upon vegetables; and on the abſorption of certain powerful mineral ſubſtances into the organic ſyſtem of vegetables. In numerous inſtances I have ſubjected the ſtems and leaves of plants, young and old, large and ſmall, to the influence of the ſulphates of iron and copper. I have found, that both of theſe metallic ſalts are very *greedily* abſorbed by vegetables, inſomuch that I have detected the preſence of iron in the veſſels of a branch of mulberry, at the height of five or ſix feet above the place of immerſion in a ſolution of the ſulphate of this metal. A full account of my experiments, I deſign to communicate to the public in two memoirs. Permit me to obſerve, in the meanwhile, that the ſulphate of iron, applied to vegetables in the manner I have mentioned, " is only (to uſe your own words) a poiſon, like almoſt every thing elſe, from the over-doſe." *In ſeveral of my experiments, the branches of vegetables that were placed in veſſels containing ſolutions of ſulphate of iron and copper, lived longer and exhibited more ſigns of vigour, than ſimilar branches that were placed in equal quantities of ſimple water.* It is true, that, in many other experiments, theſe metallic ſalts proved fatal to my plants; but this was when I employed too large a doſe. In like manner I had found ſeveral years ago*, that camphire, by greatly ſtimulating, often kills vegetables; and yet, when properly doſed, this is a very wholeſome ſtimulant to plants. I had alſo found, that large doſes of nitre (which is unqueſtionably a powerful ſtimulant both with reſpect to animals and vegetables) produce an appearance like genuine gangrene in the leaves of vegetables: and yet it is certain that nitre, when it is judiciouſly doſed, may be made to greatly aſſiſt the healthy vegetation of plants.

Excuſe the liberty I have taken in troubling you with theſe few looſe hints, and permit me to ſubſcribe myſelf,

Sir, your very humble and obedient Servant, &c.

BENJAMIN SMITH BARTON.

4. *Sulphate of Iron in the Peat of Ruſſia, found by Profeſſor Robiſon.*

Sulphate of iron exiſts in Ruſſian peat.

Something elſe beſides vegetable matter is neceſſary to form peat or black moſs of the moors. The ſmell of burning peat is different from that of vegetable matter. Peat aſhes, ſays the Profeſſor, always contain a very great proportion of iron; he has ſeen three places in Ruſſia where there is ſuperficial peat moſs, and in all of them *the vitriol is ſo abundant as to effloreſce.* In particular, on a moor near St. Peterſburgh, the clods ſhew the vitriol (ſulphate of iron) every morning when the dew has evaporated. According to this learned Profeſſor's obſervation, the ſulphate of iron in pit coal may be accounted for in the following manner: " peat moſſes form very regular

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[*] See Tranſactions of the American Philoſophical Society, Vol. IV. No. xxvii.

ſtrata, lying indeed on the ſurface; but if any operation of nature ſhould cover this with a deep load of other matter, it would be compreſſed and rendered very ſolid: and remaining for ages in that ſituation, might ripen into a ſubſtance very like pit coal. See the Medical and Chirurgical Review for November 1803.

5. *Mr. Anſtey's Teſtimony of the Uſe of Peat Duſt and Peat Aſhes.*

SIR,

Houghton Regis, Dec. 3, 1801.

Teſtimony of Mr. Anſtey.

I received yours, dated the 18th of November laſt, in which you requeſted me to inform you what experiment I had made from the turf duſt, taken from Tingrith Moor. I have made uſe of the aſhes and duſt near thirty years, and I frequently lay on from eighty to a hundred buſhels per acre. Our land is dry and very thin ſtapled, owing to the chalk rock laying ſo very near the ſurface; it encourages vegetation in moiſt warm weather; but when hot and dry, the reverſe. We never mix any other manure with it. It coſts about four-pence per buſhel, including all expences.

We chiefly ſpread it on our ſeed graſs, clover, &c.

I am, Sir, your humble Servant,

JOS. ANSTEY.

IV.

On Spectacles. In a Letter from Mr. EZEKIEL WALKER.

To Mr. NICHOLSON.

DEAR SIR,

Lynn, February 16, 1805.

Poſition reſpecting ſpectacles,

IN a former paper, I pointed out a property in ſpectacles, which had been overlooked by the writers on optics; but as ſome of your readers entertain opinions contrary to mine, the ſubject ſeems to require further inveſtigation.

viz. that we ſee better by oblique, than by direct rays.

What I have advanced concerning ſpectacles is, that we ſee better by oblique, than by direct pencils of rays[*].

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[*] Philoſophical Journal, Vol. VII. p. 291.

It has been demonſtrated by mathematicians, that an oblique pencil of rays has its focus a little nearer the lens, than a direct pencil; but how much this difference amounts to, at different angles, ſeems not to have attracted their attention. This difference, however, is too conſiderable to be diſregarded, when the rays of light fall very obliquely.

Conſiderations relating to oblique pencils.

For let L L (*Fig.* 3. *Pl.* xi.) repreſent a double convex lens, F its focus, A F its axis, and *a b* a ray parallel to it, which will be refracted to F, after paſſing through the lens. Alſo let *c b* and *d d* be two oblique rays falling upon the lens, at *b* and *d*.

Then the rays near A F falling almoſt perpendicularly upon the ſides of the lens, are leſs bent than the rays *c b*, *d d*, which fall obliquely on both ſides of the lens. "For the more oblique they are, the more they are bent, and turned out of their direct way. And conſequently the oblique rays *c b*, *d d*, will ſooner interſect in G, than thoſe at F." (Emerſon's Optics, page 124.) When the angle *a b c* is very conſiderable, the difference between *x* F, and *y* G, will likewiſe be very conſiderable. And the following ſhort account of my experiments will throw ſome light upon the truth of this propoſition.

Spectacles obliquely placed.

In the ſpectacles that I uſe, the reading diſtance of oblique rays is four inches nearer the glaſſes, than the diſtance at which I can ſee beſt, with the direct rays.

And by experiments made with a lens of 24 inches focal diſtance, and 4 inches in diameter, contracted to ¾ of an inch, I found the focal diſtance of an oblique pencil of rays 8 inches ſhorter than the focal diſtance of the direct rays. The axis of the lens, when placed for the oblique rays, made an angle of 26° with a line drawn from its centre, to the candle with which the experiments were made. And the diſtance between the candle and the lens meaſured about 10 feet, when the lens was placed to receive the direct rays.

Cor. 1. When an object is ſeen through ſpectacles, by oblique rays, it appears larger than by direct rays.

For the object is ſeen under the greateſt angle, by thoſe rays that are moſt refracted.

Cor. 2. When an object is viewed by oblique rays through ſpectacles, it is ſeen more diſtinctly with one eye, than with the other.

For the rays fall more obliquely upon one of the glaſſes, than upon the other.

Cor. 3. The direct pencils of rays are but ſeldom uſed, by a perſon whoſe ſpectacles are of the longeſt focal diſtance, that will afford him diſtinct viſion.

For he generally looks through thoſe parts of his glaſſes, which are remote from their centres, in conſequence of that motion which is given to the eyes, to view objects in different directions.

I am, Dear Sir,
Your moſt obedient Servant,
EZEKIEL WALKER.

V.

Remarks on certain Paſſages in Dr. Thomſon's Chemiſtry, together with ſome Experiments on Sandarach and Maſtic. In a Letter from Mr. R. MATTHEWS.

To Mr. NICHOLSON.

SIR,

IN a work of ſuch importance as Dr. Thomſon's Chemiſtry, which muſt be ſo frequently referred to by the lovers of that ſcience, the ſlighteſt inaccuracy ought not to paſs unnoticed: the following obſervations therefore, perhaps may be conſidered ſufficiently intereſting to deſerve a place in your valuable Journal.

In Dr. Thomſon's chemiſtry,

In his 4th Vol. page 307, Dr. Thomſon has the following remark: "Hitherto it has been affirmed by all chemiſts both ancient and modern, that the alkalies do not exert any action on the reſins." This remark I admit may be generally true; but although moſt chemiſts have been negligent in aſcertaining the effects of alcalies upon ſubſtances of this nature, I conceive ſome of them were not unacquainted with the power they poſſeſs of diſſolving reſinous bodies.

it is ſtated, that Chemiſts formerly aſſerted, that alcalies do not act upon reſins.

Boerhaave, for inſtance, in his elements of chemiſtry, page 545, expreſsly ſays, that, "both the fixed and volatile alcalies have a diſſolving power, upon animal, vegetable, and mineral ſubſtances, ſo far as theſe contain oils, balſams, gums, *roſins*, or gummy roſins, &c."

The contrary is aſſerted by Boerhaave;

and by Hoffmann.

Hoffmann alſo, by the method he employed in diſſolving amber with cauſtic nitre, evinced that he was acquainted with this property in alcalies. Dr. Thomſon deſcribes Mr. Hatchett as the firſt diſcoverer of the action of alcalies upon reſin. The facts above quoted will leſſen the originality, without diminiſhing the value of Mr. Hatchett's ingenious experiments.

How therefore it can be true that, " all chemiſts both ancient and modern, have affirmed, that the alcalies do not exert any action on the reſins" I cannot eaſily diſcern, unleſs indeed we impute to them the moſt palpable contradictions, an error which I believe is not often found in their writings.

Dr. Thomſon aſſerts, that one fifth of ſandarach is inſoluble in alcohol.

Page 340 in the ſame volume, Dr. Thomſon aſſerts, that ſandarach is not ſoluble in alcohol, about a fifth part of it remaining undiſſolved; and deſcribes the inſoluble part as poſſeſſing peculiar properties, differing from ſandarach, and calls it *ſandaracha.* It ought not to be concluded, that Dr. Thomſon himſelf can have chemically examined every ſubſtance he muſt have occaſion to deſcribe in a work which embraces ſo great a variety; but the importance of ſandarach as an ingredient in varniſhes being nearly equal to that of any other reſin, this ſubſtance certainly deſerves more attention from chemiſts than has hitherto been beſtowed on it. The properties of this reſin as mentioned by Dr. Thomſon, are ſtated to reſt ſolely on the authority of Gieſe, no other chemiſt having examined it.

Experiment. It proved ſoluble in eight times its weight of alcohol.

Upon trial I found that ſandarach was ſoluble in eight times its weight of alcohol, a very minute proportion only being left, which appeared to conſiſt almoſt entirely of extraneous matter. Conſequently the ſubſtance Dr. Thomſon has denominated *ſandaracha* can have no exiſtence in this reſin. It is proper to obſerve, that the reſin I made uſe of was ſelected fine, and is known in commerce by the names of gum juniper and ſandarach. From the obvious diſagreement in the reſults, perhaps we ought to conclude, that the ſubſtance employed by Gieſe in his experiments was not true gum ſandarach *.

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* Looking to ſome chemical notes, I find that when three or four tears of ſandarach were put into pure alcohol, and left for a day, a ſmall quantity of thick fluid remained at the bottom, which, however, was taken up by agitation, and the whole became ſemi-opake:—It is remarkable, that this ſubſtance is not ſoluble in tallow or oil, as common reſin is. N.

Speaking of maſtich, vol. 3, p. 311. Dr. Thomſon ſays, it is readily ſoluble in alcohol. In the ſame valuable volume, page 332, he informs us, that "that part of maſtich which is inſoluble in alcohol is ſaid to be pure caoutchouc." In inveſtigating this apparent contradiction, I examined the action of alcohol upon maſtich, and found that nearly a fifth part of it remained undiſſolved. This reſiduum, after repeated waſhings in alcohol, was white, conſiderably elaſtic, and adheſive; it was inflammable; and when heated became brown, emitting inflammable gas. In this ſtate it had much the appearance of common Indian rubber, but ſomewhat glutinous: It was not in the leaſt acted on by water.

Dr. Thomſon's account of maſtich.

The Author's experiments.

Alcohol leaves one fifth of maſtich undiſſolved, reſembling Indian rubber.

I was induced to try the effects of different menſtrua on this reſiduum, and on the elaſtic gum caoutchouc, with a view to aſcertain their identity or otherwiſe. The following are the reſults I obtained:

Comparative experiments on the reſidue of maſtich, and on caoutchouc.

Sulphuric ether, previouſly waſhed with diſtilled water, diſſolved this reſiduum as well as caoutchouc.

Alcohol precipitated both theſe ſubſtances from their ſolvent in the form of a white curd.

Water had no action on the ſolutions.

By the nitric acid the reſiduum was converted into a yellow brittle porous maſs, nitrous gas being diſengaged from the acid.

Nitric acid did not act ſo eaſily on caoutchouc; but when raiſed to the boiling point it changed it into a ſimilar ſubſtance.

With the ſulphuric acid a ſubſtance like charcoal was formed, the acid aſſuming a dark port wine colour, part of it being converted into ſulphureous acid.

The ſame effect was produced by the ſulphuric acid on caoutchouc, though not without the aſſiſtance of heat.

Neither the muriatic or oxymuriatic acids appeared to have any action on either of theſe ſubſtances.

Acetic acid diſſolved a ſmall portion of the reſiduum, but had no action on caoutchouc,

Solutions of potaſh and ammonia produced no effect on either the reſiduum or caoutchouc.

From the above experiments it will be obſerved, that the acids act with greater facility on the inſoluble part of maſtich, than on caoutchouc. Notwithſtanding this difference in their

Theſe appear to be the ſame.

action, I think it may be inferred, from the coincidence in their effects, that theſe bodies are ſimilar ſubſtances. Probably the action of the acids on the reſiduum was aſſiſted by a ſmall portion of reſin which it was combined with, and which it defended from the action of the alcohol employed to diſſolve the maſtich.

I am, your obedient Servant,
R. MATTHEWS.

March 13, 1805.

VI.

The Dutch Method of curing Herrings, extracted and tranſlated from the German of Krünitz's Economical Encyclopædia (Oeconomiſche Encyclopädie), *Article Häring, by* J. HINCKLEY, *Eſq. F. S. A.*

(Concluded from p. 224.)

Dutch method of catching and curing herrings.

HERRINGS, however, are equally good, wherever they are taken, provided they be but caught in the proper ſeaſon, and well managed. As they die immediately on quitting their element, ſalting and packing are the circumſtances which principally affect their quality. The ſuperior excellence and flavour of Dutch herrings, above thoſe of all other countries, ariſes from the cloſe attention and indefatigable induſtry employed. Every thing, however minute, both as to ſeaſon and management, which can maintain the reputation they have enjoyed for more than two hundred years, is moſt punctually obſerved; and above thirty ordinances on the curing and management of herrings are recited by Sir William Temple, in the 17th century.

Herrings, cured with Scotch ſalt, very quickly decay. Thoſe of Norway are cured in the ſame manner as the Scotch, but with French ſalt, and packed in fir or deal; in conſequence of which they are worſe, and leſs palatable, as they leave a ſour taſte in the mouth, and ſoon ſpoil. In like manner, other nations are equally careleſs in aſſorting the fiſh, and to this may be attributed the precedency which the Dutch herrings have ſo long maintained. The Dutch catch their fiſh regularly and early off Hittland, from the 25th of June,

Dutch method of catching and curing herrings.

becauſe they are then, and to the beginning of July, fatteſt; after which time, the nearer they approach the coaſt, the leaner and worſe they are.

It redounds no leſs to the honour of the Dutch, than to their advantage, that they pay the utmoſt poſſible attention to theſe rules: according to which, the fiſh muſt be taken at the proper ſeaſon, properly ſalted, well aſſorted, and rightly packed; to do all which, the captain and ſailors are by ſeveral laws obliged to bind themſelves by oath, before they ſail. There are alſo overſeers well paid, that they may not betray their truſt, but watch and enforce every the minuteſt regulation; to which circumſtance alſo, the pre-eminence of Dutch herrings throughout the world may partly be aſcribed.

As ſoon as the herrings are taken out of the water, they are thrown either upon the end of the deck, which has been cleared, and made perfectly clean for the purpoſe, or into baſkets; and then *gipped* (the gills and guts taken out) with a knife, by ſome of the crew, who are ſolely employed therein, having been brought up to that practice. The milt, or roe, however, is always left in the fiſh.* What are taken during one night, are, before the following ſun-ſet, neatly and ſkilfully laid in oaken barrels, coarſe Spaniſh or Portugueſe bay ſalt being ſtrewed between. This the fiſhermen of other countries either entirely neglect, or leſs carefully perform, being leſs ſcrupulouſly nice; becauſe they either go out to ſea later, or, like the Scotch, commence fiſhing too ſoon; or only navigate ſmall boats near the coaſt; do not kill the fiſh with a knife, or gut, ſalt, or pack them, while on board, but throw them down in a boat, and when fully laden, go on ſhore, proceed at their leiſure, caſt the fiſh on the ſea-coaſt in conſiderable heaps, where they are even ſuffered to lie ſometimes ſeveral days, before they are gutted, ſalted, and packed, in conſequence of which they grow ſtale and ferment. The Dutch, on the contrary, indefatigably purſue their method day and night, during twenty, twenty-four, or twenty-ſix weeks, be the weather what it may. Hence, their fiſh are uſually more tender, better flavoured, and not ſo very ſalt as the Engliſh and the Scotch.

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* Mr. M'Culloch's Treatiſe (ſee note, page 413) ſays, they ſhould alſo be kept cleanly, and out of the ſun, as well as froſt or rain.

There

Dutch method of catching and curing herrings.

There are two methods of ſalting and preſerving herrings for a conſiderable length of time. The one is called white ſalting, the other red. The former is thus performed. Immediately on being taken, the fiſh are gutted, as above deſcribed, and waſhed in clean water. Then ſalt is ſprinkled on them, either internally, or both within and without, and the fiſh, being thrown into large baſkets with handles, are well rouſed, (or ſhaken about a few times) that the ſalt may the better diffuſe itſelf, and penetrate: or, laſtly, which is the beſt method, they are thrown into a tub filled with a ſtrong brine made with bay ſalt and freſh water, in which an egg will ſwim. In this pickle they are left upon deck in the open air, provided the weather be good, during twelve or fifteen hours; but, if circumſtances require, a good deal longer, and are well ſtirred (eſpecially if pickled on ſhore) with ſhovels ſeveral times, that the ſalt may the better and more generally penetrate every where. Laſtly, to pack them properly, they are taken out of the pickle, ſuffered to drain ſufficiently, and then packed in barrels, which are ſtrewed at bottom pretty thickly with ſalt, and, if there is time enough, they are neatly laid in ſtrata or layers, always ſtrewing ſufficient ſalt upon each layer. But, if the abundance of fiſh be too great, they are thrown in promiſcuouſly, with as much ſalt as is requiſite to preſerve them from ſpoiling. When this is done in ſtrata, each new layer is preſſed down hard upon the preceding. This laſt-mentioned proceſs, however, cannot eaſily be attended to at ſea, eſpecially when the fiſhery is very abundant. Hence, if the fiſh are to be exported, or remain long unuſed, they are re-packed on ſhore, laid in freſh ſalt, and preſſed down hard; without which precaution herrings exported by ſea would ſpoil. The ſame practice is purſued in France, Hamburgh, and doubtleſs elſewhere. At Hamburgh, as in Holland, they are packed in the open air, ten packers and three overſeers being appointed for the purpoſe, and all ſworn. Beſides re-packing, the packers in the maritime towns have alſo to pick and aſſort the fiſh, according to their goodneſs, ſalt them anew, and put them in freſh pickle. It is alſo generally a part of the magiſtrates and trading companies oaths, not to ſuffer any bad fiſh to be delivered from the quay or cuſtom-houſe.

After packing, whether the goods are intended for exportation or home-conſumption, whether preſſed down hard or not, they are regularly coopered, that the pickle may not leak out, and the fiſh turn yellow or ſpoil, which takes place the moment they are deficient in pickle. Properly, the herrings ſhould, on the very day on which they are taken, not only be gutted, but ſalted and put in caſks, or at leaſt ſhould not lie more than one night in the firſt pickle. And accordingly ſuch herrings are diſtinguiſhed in France by the name of *Harengs d'une nuit*. But, when the fiſhery is abundant, this is not always poſſible; ſo that only a part can be properly attended to; and the reſt, after being gutted, muſt unavoidably remain, at leaſt the whole following day, if not longer, in the firſt pickle, the regular packing of them being poſtponed till the third day. Theſe fiſh, having ſtood two nights on deck in the open air, are called *Harengs de deux nuits*. But ſuch goods not only are inferior, but do not keep ſo well as the former.

Dutch method of catching and curing herrings.

If the fiſhermen mean again to caſt their nets on the following night, or if, on account of the great abundance of the ſhoals, they do not expect to complete the ſalting and regular packing in two days, the fiſh, which they cannot ſo complete, are ſalted in large heaps, and are then called *Slabbers*, or *Slabbegut*, coarſe goods. Theſe are frequently too ſalt, becauſe want of time prevents their being properly managed. They are put into the ſchuyts, which always accompany the herring-buſſes, and waſhed; after which they are ſmoked; though not ſo much as the *Bicklings* (Bücklinge) or red-herrings. The ſalted herrings, hitherto ſpoken of, are called *Böckel herringe*, or pickled herrings, or, in general, plainly *Herrings*; thoſe properly ſalted and packed in layers, *Packed*, or *Barrel herrings*; and thoſe half ſalted, and promiſcuouſly packed in barrels, *Wrack herrings*.

The other mode of curing, called Red ſalting, is thus performed. When the fiſh are taken out of the above-deſcribed pickle, in which, however, they muſt remain longer than thoſe intended for the common, or white-ſalting, and at leaſt four-and-twenty hours; they are hung by the head in rows on wooden poles, in ſtoves conſtructed for the purpoſe, each of which generally contains 12,000 herrings. Being thus placed, a fire is made under them with vine-ſtalks, or any green

Dutch method of catching and curing herrings.

green ſaggot-wood, that affords much ſmoke and little flame.* Here they remain till dried, and properly ſmoked, which generally requires twenty-four hours. Thus they become Bicklings, or Red-herrings; when theſe are packed in barrels, they are called Barrelled Bicklings, and are much ſalted; but, if laid in ſtraw, they are called Straw Bicklings, and are ſomewhat leſs ſalted. The excellence of the Bicklings principally conſiſts in their being large, fat, tender, freſh, properly ſalted, pliable, ſoft, of the colour of gold, and not torn or mangled. In Holland, the beſt fiſh are choſen for this purpoſe; but in other places, the above-deſcribed Slabbers only are uſed, or other inferior herrings, deemed unfit for the uſual mode of ſalting. The beſt and fatteſt ſmoked Dutch herrings are called, in German, *Speckbucklinge*, or fat red-herrings; in low Saxon, *Flickhäringe;* and in Hamburgh, *Fläkheeringe*. They are cut open along the back.†

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* At Bremen, the place moſt celebrated for ſmoking fiſh, and where no ſecret is made, as in Holland, of the proceſs, they are hung in ovens of the ſize of a ſmall parlour, and ſtrict attention is paid not to uſe fir, or any wood, in which is any the leaſt turpentine or reſinous matter, which invariably gives a bad taſte to the fiſh.

† The act of parliament regulations, and many uſeful obſervations, may be ſeen in two ſmall tracts, 8vo, in poſſeſſion of the Society for the Encouragement of Arts, Manufactures, and Commerce, the one entitled, "Obſervations on the Herring-Fiſhery upon the North and Eaſt Coaſts of Scotland, &c.; with plain Rules, propoſed for curing, and for ſupplying the London Market with White Herrings: By Lewis M'Culloch, many Years employed in furniſhing the Merchants of London with Herrings for Exportation. London. Richardſon, 1788." The other entitled, "The beſt and moſt approved Method of curing White Herrings, and all Kinds of White Fiſh; containing particular Directions how to ſlit, gut, ſalt, dry, and barrel them, fit for Sale at home or foreign Markets; with Directions for boiling of Oil: By a Trader in Fiſh. London. J. Davidſon, 1750."

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VII.

Th Denſity of Mercury in its ſolid State aſcertained. *By Mr.* JOHN BIDDLE.

To Mr. NICHOLSON.

SIR, *Birmingham, March* 4, 1805.

AT the Philoſophical Society in Birmingham, of which I am a member, I read a few weeks ago a lecture on mercury. In the courſe of my experiments on this ſubject, conducted with a view of exhibiting this metal in a ſolid ſtate, by expoſure to the frigorific mixture of muriate of lime and ſnow, it appeared to me the ſpecific gravity of this metal would be moſt accurately obtained in compariſon with other metals under ſimilar circumſtances of ſolidity and temperature. I therefore undertook, amongſt a variety of others, the following experiments, to aſcertain its ſpecific gravity in a ſolid ſtate, which I believe had never before been accompliſhed: They ſucceeded to my ſatisfaction, and to the gratification of ſome who were preſent: I am, therefore, inclined to believe, you will think them worth inſerting in your Philoſophical Journal.

Experiments before the Philoſophical Society in Birmingham.

I am very reſpectfully yours,

JOHN BIDDLE.

HAVING purified mercury, by diſtilling from an earthen retort into a glaſs receiver 50 *per cent*. of the quantity put in, to avoid the alloy of any other metal, and afterwards having heated it to 300° of Fahrenheit, to drive off any water that might adhere, ſuch as was uſed in other experiments, and expoſed to the action of muriate of lime and ſnow,—I proceeded as follows:

Mercury was purified by diſtillation;

One thouſand grains of this mercury were introduced to the freezing-mixture, together with three ounces of alcohol, in a round-bottomed glaſs; and having placed in the mercury a fine bent wire, the weight of which was previouſly known when immerſed in the ſame alcohol, to a certain point, in the temperature of 47 degrees above zero of Fahrenheit: this wire, during the congelation of the mercury, was fixed in it, ſo that

and expoſed to congeal under alcohol.

that the whole became attached to the inner ſurface of the glaſs, and could not be ſeparated until the glaſs with its contents was taken out of the freezing-mixture, and warmed, by dipping it in water, ſo as to ſoften the mercury at the ſurface attached to the glaſs; one hand being applied to the wire at the ſame time, to draw the mercury from the glaſs at the moment of its being looſened: it was then replaced, as before, in the freezing-mixture.

Viſible ſigns of great contraction of the mercury by freezing.

During the congelation of the mercury it was obſerved, the ſurface of the metal towards the centre was very conſiderably depreſſed by the contraction of its particles, and the veſſel having been moved during its congelation, a ſmall hole nearly reaching the bottom of the glaſs, was obſerved gradually leſſening in its dimenſions, preſenting a conical cavity with the apex downwards. The wire which held the mercury attached to it was now fixed by the other bent end to the hydroſtatic balance, which held in the oppoſite ſcale the weights by which the mercury had been weighed in the air, together with the balance of the wire immerſed in the ſame alcohol to a point marked on the wire.

Loſs of weight by the immerſion.

It appeared by weighing the 1000 grains of mercury thus immerſed in alcohol, the loſs of weight was 59,8 grains: this was weighed five or ſix times, with the glaſs ſtill in the freezing mixture, with the ſame reſult; but when it was withdrawn only a little way from the muriate of lime and ſnow, the difference of weight was perceptible, in conſequence of the temperature of the mercury and alcohol increaſing.

Comparative experiment with an equal weight of ſilver.

One thouſand grains of pure ſilver, weighed by the ſame ſcale immerſed in the ſame alcohol at the ſame temperature, loſt in weight 88,105 grains: therefore, as the loſs of the weight of the mercury is to the loſs of weight in the ſilver, ſo is the ſpecific gravity of the ſilver to the ſpecific gravity of the mercury.

Whence the ſpecific gravity of ſolid mercury is deduced = 15.612.

The ſpecific gravity of the ſilver in the ſame balance having been aſcertained, by diſtilled water, to be 10,436, it follows, if this ſum be multiplied into the ſum of its loſs ſuſpended and weighed in alcohol, and divided by the ſum of loſs of mercury weighed in the ſame way, that the ſpecific gravity of the mercury in the ſolid ſtate, at about 40 degrees below zero on Fahrenheit's ſcale, equals 15,612.

By the ſame hydroſtatic balance it appears, the ſame mercury in a fluid ſtate, the thermometer ſtanding at 47 degrees above zero, is 13,545.

Sp. Gr. when fluid, was 13,545.

By theſe experiments it appears, the difference of denſity between its fluid ſtate, at the temperature of 47° above zero, and its ſolid ſtate, at 40° below zero, is 2,0673 in 13545, or 1,5265 in 10, which is 15,265 *per cent.* that is, nearly $\frac{1}{7}$th of its greateſt volume, or nearly $\frac{1}{6}$th of its leaſt volume.

VIII.

Obſervations on Mr. Boswell's *Geometrical Propoſitions. By* An Old Correspondent.

To Mr. NICHOLSON.

SIR, *March* 6, 1805.

WHEN your ingenious correſpondent Mr. Boſwell ſent to you his diſcovery of a ready method of determining a chord-line equal to the ſide of a ſquare of the ſame area as the circle in which it is drawn, it ſhould ſeem that he was not aware that the quantum of error might be aſcertained by calculation, as you have done in the ſubjoined note, otherwiſe he would not have ſatisfied himſelf with the mechanical proof which he has adduced as a teſt of its accuracy. Had it occurred to you, I am perſuaded you would with equal facility have ſhewn, that this gentleman's other "fact in geometry" is ſtill more erroneous, though announced with a degree of confidence which appears to have ariſen from a miſtaken conviction of ſuch obvious accuracy as ſtood not in need of proof.

On Mr. Boſwell's lines drawn in a circle.

"*A right line* (B E), ſays he, *drawn from the extremity* B *of the line* I B, *at right angles through the oppoſite diameter* (I F) *to the circumference, will be equal to a fourth of the circumference.*"

Let us try how far this aſſertion is founded in truth. But before we enter upon the examination, I will beg leave to point out a circumſtance in your note, page 152 in your laſt number, which, having ariſen out of inadvertence in your tranſcribing

Overſight by inverſion of terms in W. N.'s note.

Farther elucidation of the note.

tranſcribing it moſt probably, may tend to puzzle your readers, and among the reſt perhaps Mr. Boſwell himſelf, who muſt be the moſt intereſted: You have ſaid, "as the radius is to I W, ſo is the diameter to I B," (Plate VIII. Fig. 10); but I take it for granted you meant, "*as* I W *is to radius, ſo is the diameter to* I B," becauſe this analogy is agreeable to the reſult of your calculation. And as you have not mentioned how the doctrine of ſimilar triangles applies in the conſtruction of the figure, I hope you and your numerous readers will not deem me preſumptuous, if I introduce the diagram again with ſome additions, particularly as it will give me an opportunity of proving more intelligibly the error of the ſecond propoſition already quoted. Any triangle formed in a ſemicircle, of which the diameter is one ſide, with its oppoſite angle at the circumference, is a right angled triangle (Eucl. III. Prop. xxxi.): This will be the caſe in Mr. Boſwell's figure if a right line be drawn from B to F, the right angle being at B; and as the angle at I is common to the ſmall right angled triangle I O W, and to the large one I B F, the two triangles are ſimilar, and therefore the hypothenuſe I W of the ſmall one is to its baſe I O, as the hypothenuſe I F (or diameter) is to the baſe I B of the large one. Q. E. D.

How far Mr. B.'s ſecond propoſition is accurate.

With reſpect to the ſecond propoſition, it is very well known (Eucl. III. Prop. xx.) that the angle formed at the centre of a circle is double of the angle at the circumference, if they have the ſame common chord as a baſe; now in the triangle I O W (Plate XI. Fig. 4), the ſides I O being given $= .5$, $O\,W = .25$, and therefore $I\,W = .55901$, we have, by a ſimple caſe in plane trigonometry, the angle at $I = 26^\circ\ 33'\ 5''.4$, the double of which is $53^\circ\ 6'\ 10''.86$ for the angle B O F at the centre; but B G, one half of the line B E in queſtion, is the ſine of this angle, and in natural numbers is, by the tables, equal to .7997163, when unity is radius; but, in our figure, the diameter repreſents unity, therefore the double ſine, or chord line, B E is $= .7997163$, which, according to Mr. Boſwell's aſſertion, ought to be $= .7854$, or one-fourth of the circumference 3.1416; hence the error is .0143163, or very nearly $\frac{1}{70}$th of the whole.

Apology to theſe remarks.

As I have not the pleaſure of knowing Mr. Boſwell, notwithſtanding I have admired ſeveral ſpecimens of his ingenuity, and as I profeſs to be a promoter of all arithmetical approximations and ſcientific projects, that have for their means facility of application, and utility for their object, I mean ſo far as they have accuracy to recommend them, I truſt he will not impute any motive of a perſonal nature to me, but give me credit for the declaration, when I aſſert, that the implied invitation collected from his memoir, to aſcertain the accuracy of any of his projects, has been my only inducement for laying before the public the preſent paper.

I am, Sir,
With due reſpect,
AN OLD CORRESPONDENT.

IX.

A Memoir on Milk and the Lactic Acid. By Cit. BOUILLON LAGRANGE.

(Concluded from page 144.)

THE muriate of ſoda, and more eſpecially the muriate of lime, affords a proof of this. Theſe ſalts effect a ſeparation of the curd only in part; and, nevertheleſs, the muriate of lime has a very great affinity for water: the decompoſition ought therefore, in this caſe, to be made in a more ſtriking manner than with the acids.

Curd is not ſeparated by the mere abſtraction of water.

Whey is not therefore the product of fermentation. The alcohol obtained from milk does not prove that this formation is neceſſary in order to obtain the lactic acid; for this laſt may be obtained from a ſerum recently prepared.

Whey not a product of fermentation.

With regard to the formation of the carbonic acid indicated in the ſixth experiment, it is owing to the decompoſition of a ſmall portion of animal matter, and part of the ſugar of milk diſſolved in the ſerum. The more of theſe ſubſtances is decompoſed the greater will be the quantity of carbonic acid formed, and accordingly the degree of acidity is more marked on account of the preſence of the carbonic acid; but if the acid be expelled, the ſerum is milder, and not ſo ſour as that which has been for ſome time expoſed to the air.

Carbonic acid in whey.

If the ſerum be expoſed to the air for ten or twelve days, in proportion as the ſaccharine matter is decompoſed the whey becomes

Acetic acid.

becomes more acid. Theſe degrees of acidity are owing to a ſmall quantity of acetic acid, formed by the aid of carbonic acid and alcohol remaining in the liquor, which muſt naturally augment the acidity of the acid already formed in the milk.

Whether the ſalts in whey render it ſour.

With regard to the ſecond queſtion, Whether whey exhibits acid properties only on account of the ſalts it holds in ſolution?

I have proved that the acid may be ſeparated from the ſaline matters without expoſing the milk to the air for a certain time, as Scheele has directed; and the experiments I ſhall relate in this memoir will afford additional proofs. So that its acid properties do not depend merely upon the ſalts it holds in ſolution, and the lactic acid is not produced by a fermentation ſimilar to the acetous.

Zoonic acid by diſtilling milk;

Though the action of caloric upon milk is generally known, I ſhall nevertheleſs make an obſervation upon the acid obtained by diſtillation with a naked fire, which has for ſeveral years been known by the name of the Zoonic Acid.

is acetous acid.

If this acid be purified and combined with potaſh, a ſalt is obtained, which preſents all the characters of the acetate of potaſh.

Curd contains phoſphate of lime.

I muſt alſo obſerve the cheeſy matter in which ſome chemiſts have been unable to find phoſphate of lime, as announced by Scheele. To obtain this, nothing more is neceſſary than ſtrongly to heat this ſubſtance in a crucible after having well waſhed it, and to calcine it to whiteneſs.

The reſidue is a hard white matter, of which ſome ſmall pieces, veined with blue, may be compared to the turquois.

The nitric acid diſſolves it without effervеſcence; lime-water and ammonia occaſion a flocculent precipitate of phoſphate of lime.

The oxalate of ammonia alſo demonſtrates the preſence of lime.

SECTION III.

Concerning the Serum of Milk.

Examination of whey ſeparated by different agents.

Being deſirous of knowing whether there is any difference between the wheys obtained from milk by different ſubſtances, I made uſe of the mineral acids, ſome vegetable acids, ſalts, ſuch as alum, the acidulous tartrite of potaſh, &c. The coagulating ſubſtance was employed in due quantity; as I had remarked, that when an exceſs of acid is uſed, it is found to exiſt in the ſerum; whereas, when the neceſſary quantity is uſed, it is found in the cheeſe only.

It is the ſame fluid in the different caſes.

After having ſeparated the cheeſy matter the ſerum was clarified; the wheys were found to have all the ſame taſte and colour, and poſſeſſed the ſame degree of acidity.

It affects ſyrup of violets.

This freſh whey turns ſyrup of violets green: This property muſt be attributed only to the union of the yellow and blue, and not to ſaline matters, as chemiſts have aſſerted. Whey, which has loſt its colour by expoſure to the air, reddens the ſyrup of violets.

The means employed to aſcertain the acids which had been uſed to coagulate the milk, did not exhibit even the ſmalleſt quantity when the operation was performed with exactneſs.

Sour ſerum by expoſure of milk

I made a comparative examination of the ſerum which had become ſour in the open air; and I obtained, by expoſing the milk, the ſame phenomena as were pointed out in the ſecond ſection and third experiment.

differed from other ſerum.

The ſpontaneous ſerum differed, 1. In its taſte, which was more acid; and, 2. Its colour, which is dull and whitiſh, owing to white filaments which are ſeparated. It may be had clear by filtration, without acquiring the colour of freſh whey.

Cheeſy matter:

The cheeſy matter reſulting from the ſeveral experiments before related, gave the ſame red with tincture of turnſole. If it be calcined and treated with the known re-agents, the preſence of ſulphuric acid will be indicated if this acid has been employed in the coagulation, and alumine when alum has been made uſe of.

Alum or ſulphuric acid may be employed to coagulate milk.

I do not therefore ſee any inconvenience, in preparing whey for the purpoſes of pharmacy, to coagulate the milk with one or the other of theſe ſubſtances, more eſpecially if the precautions I have mentioned be attended to. Thoſe who have thought it unwholeſome to coagulate milk with alum, have doubtleſs made no experiment on the ſubject.

Oxigen and ſerum.

Oxigen gas is not abſorbed by the ſerum; even agitation does not facilitate its union.

Other agents.

The action of lime-water, the ſolution of barytes, ſtrontian, and ſome metallic ſalts, though already known by chemiſts, require to be again examined, and will enable me to explain the proceſſes for obtaining lactic acid.

SECTION IV.

On the Sugar of Milk.

Whether acetic acid in milk ariſe from the decompoſition of its ſugar.

The formation of acetic acid does not ariſe merely from the alteration of the ſugar of milk; and though the experiments already deſcribed admit of certain conjectures, I have aſcertained whether a like quantity of ſugar of milk could be obtained from freſh ſerum and a ſerum expoſed for twelve days at a temperature from 14 to 20 degrees.

A little ſugar is decompoſed by ſtanding.

I have found that ſour milk affords a ſomewhat leſs quantity of ſugar of milk, which proves, that a portion of the ſaccharine matter was employed in forming a quantity of acetic acid.

We may therefore obtain carbonic acid, alcohol, and acetic acid, either by confining whey in cloſe veſſels, or by leaving it expoſed to the air.

Other experiments made on pure ſugar of milk indicated nothing remarkable.

Habitudes of ſugar of milk.

It does not redden tincture of turnſole. When diſſolved in water and expoſed for a long time to the air, it does not become acid. By diſtillation it affords acetic acid along with the known products.

It is ſoluble in weak acetic acid. If a ſmall quantity of freſh gaſeous matter be added, it is ſoon found that the liquor holds it in ſolution. By pouring in a few drops of alkali to ſaturate the exceſs of acid, it loſes its tranſparency, becomes turbid and milky, with a mild taſte reſembling that of milk, and preſerves its opacity for a conſiderable length of time.

General reſults reſpecting milk.

The following reſults are deducible from the preceding facts:—

1. New milk reddens the tincture of turnſole.

2. The cheeſy matter may be ſeparated without contact of the air.

3. The ſerum does not retain the acids made uſe of to coagulate milk.

4. Diſtillation either of milk or of ſugar of milk to dryneſs, affords the acetic acid.

5. The formation of carbonic acid and alcohol is owing to muco-ſaccharine matter.

6. A quantity of acetic acid is formed by the fermentation of theſe ſubſtances.

7. The cheeſy matter, when ſeparated, always manifeſts the preſence of an acid, and differs in taſte and conſiſtence according to the matters employed in its ſeparation.

8. When waſhed and affording no ſign of acidity, if it be afterwards diffuſed in diſtilled water for ſeveral days, at a temperature between 15 and 22 degrees (about 65 and 80 Fahrenheit), it acquires a ſtrong diſagreeable odour; the fluid ſlightly reddens turnſole, and lime diſengages ammonia.

9. The ſerum and the cheeſy matter contain, beſides the known ſubſtances, phoſphate of lime, as Scheele had announced.

10. The difference which exiſts between freſh ſerum and that which has been expoſed to the air, conſiſts in the acetic acid, which is preſent along with the free acid in the milk.

11. Laſtly, there exiſts in milk and the ſerum a free or uncombined acid, which ſeemed to me to be the acetic acid.

SECTION V.

Concerning the Lactic Acid.

Lactic acid, not obtained by ſimple diſtillation.

Scheele having aſcertained that it is not poſſible to obtain the acid by ſimple diſtillation, had recourſe to peculiar methods to effect this ſeparation.

Scheele's proceſs. Evaporate whey to one-eighth.

This chemiſt firſt reduced the whey to one-eighth by evaporation; he then filtered it, and there remained, according to him, no more cheeſy matter.

Filter. Saturate with lime-water, which throws down phoſphate. Filter. Dilute with water. Precipitate lime by oxalic acid. Evaporate to mucous conſiſtence. Diſſolve the acid in alcohol and filter. Then add water, and diſtil off the alcohol. Reſidue is lactic acid.

He ſaturated the fluid with lime-water, and phoſphate of lime was precipitated: This liquor having been filtrated and diluted with three times its quantity of water, he poured in oxalic acid drop by drop, to ſeize and precipitate all the lime; and he aſcertained, by the addition of a little lime-water, that no more oxalic acid remained. He evaporated the liquor to the conſiſtence of honey; the thickened acid was rediſſolved in rectified alcohol; the ſugar of milk and all the foreign ſubſtances remained on the filtre, not having been diſſolved by the alcohol. Laſtly, after having again added a little water to the acid held in ſolution by the alcohol, he drew off this laſt fluid by diſtillation, and found the lactic acid in the retort.

It is not pure.

This long and expenſive proceſs does not afford a pure acid. It was therefore neceſſary to adopt a ſimpler proceſs before its nature was examined.

With freſh whey little acid is obtained.

Experiment 1. If inſtead of expoſing whey to the air as Scheele directs, the proceſs be immediately performed on the freſh whey, a very ſmall quantity only of acid is obtained, which is leſs coloured and has a more animal ſmell.

This acid appeared to me to be uncombined in the milk, as its quantity was ſo ſmall.

If barytes or ſtrontian and ſulphuric acid be uſed inſtead of the lime-water and oxalic acid of Scheele the reſult is equally perfect.

Experiment 2. Inſtead of uſing lime-water and the oxalic acid, I uſed barytes or ſtrontian and the ſulphuric acid. By this means I obtained an acid no leſs pure than that of the Swediſh chemiſt.

Proceſs with acetate of lead unſucceſsful.

Experiment 3. I had likewiſe attempted to decompoſe a lactate of potaſh with acetate of lead, expecting to form a lactate of lead decompoſable by the ſulphuric acid; but the precipitate thus formed is not a lactate of lead, for when a ſufficient quantity of ſulphuric acid is added for the purpoſe of decompoſing it, no uncombined acid is ſet at liberty. It appears that part of the oxide falls down with the animal matter; and this compound is even to a certain degree ſoluble. It is alſo found that the fluid always retains much acetate of lead in ſolution.

We therefore ſee that though this proceſs has been pointed out as ſuperior to that of Scheele, it is certain that it cannot be employed to obtain the lactic acid. The following experiments were made on the lactic acid of Scheele.

Lactic acid of Scheele examined.

1. The intenſity of its colour may be diminiſhed by grinding a piece of red hot charcoal in a mortar, and pouring the lactic acid upon it. A quantity of ſmall flakes were ſeparated which roſe to the ſurface. The fluid was ſlightly boiled and filtered, and the acid was then leſs coloured, and had leſs odour.

2. When this acid is diſtilled in a retort acetic acid paſſes over and a thick yellow very acid matter remains.

3. It may perhaps be thought that part of this acid becomes changed into acetic acid by the aſſiſtance of caloric, as Berthollet obſerves in his Chemical Statics.

"The ternary acids, as this chemiſt obſerves, may be changed into acetic acid, when by the action of heat they abandon part of their carbon, and their elements, which reſiſt this action leſs, are ſeparated by volatilazation."

I have aſcertained that great part of this acid previouſly exiſted in the fluid.

3. The ſame phenomena take place, but more ſtrikingly, if whey be diſtilled which has been expoſed to the air at the temperature of 15 or 20 degrees (60 and 68 Fahrenheit) for about a fortnight. The firſt product is a clear tranſparent fluid of an acid taſte, and odour of alcohol, which when combined with potaſh, forms an acetate.

If alcohol be added to what remained in the retort, it becomes coloured. After decantation and diſtillation, an acid is found in the retort of a peculiar odour, animalized, and reſembling the preceding; but the acid contains a very ſmall quantity of phoſphate of lime.

It is a mixed acid.

Theſe experiments confirm the obſervations I have made: 1. That a mixed acid is obtained by the proceſs of Scheele, and 2. That an acid exiſts in milk and in whey recently prepared. It remains to ſhew the nature of that acid. For that purpoſe I prepared the lactic acid according to the proceſs of Scheele with every neceſſary precaution. I even aſcertained the purity of my alcohol, which having been diſtilled from muriate of lime, marked 40 degrees of the aerometer of Reaumur.

When diſſolved in alcohol ſome acid comes over with the ſpirit.

The lactic acid when diſſolved in alcohol was ſubmitted to diſtillation. There paſſed into the receiver an alcohol which reddened the tincture of turnſole.

Lactic acid remained.

When the acid was totally deprived of alcohol the diſtillation was ſtopped. The lactic acid of Scheele remained of a yellow colour and very ſour taſte.

It gave out ammonia by lime.

Experiment 1. I mixed quick lime with this concentrated acid, and a diſengagement of ammonia took place.*

Lactate of potaſh leaves pruſſic acid by heat.

Experiment 2. If a lactate of potaſh be formed and evaporated by a gentle heat, a brown matter is ſeparated, which is inſoluble in water. This being afterwards heated in a crucible, the ſalt ſwells up and emits an animal odour. The preſence of pruſſic acid was proved by diſſolving the remaining matter in water and adding a ſmall portion of the ſulphate of iron in ſolution.

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* I had before aſcertained the preſence of ammonia in this acid. I announced it on the 15th Nivoſe to the Society of Pharmacy. Mr. Vauquelin, who was not acquainted with this fact, informed me at the reading of the firſt part of this memoir that he had alſo aſcertained the preſence of an animal matter in the lactic acid of Scheele.

Experiment

Sulph. acid added to lactic acid diſengages acetous acid.

Experiment 3. If ſulphuric acid be added to lactic acid, or to a lactate of potaſh, a diſengagement takes place not only of acetic acid, but alſo of an elaſtic fluid, which forms a thick cloud when in contact with ammonia.

Diſtillation.

Experiment 4. When the preceding mixture is diſtilled in an apparatus proper to receive the gaſeous product in water, the diſtilled water of the bottle preſerves its tranſparency; the ſmell of acetic acid is very evident; it reddens turnſole and precipitates the muriate of ſilver.

When a ſmall quantity of ammonia was brought near the tube out of which the gas iſſued, a very denſe cloud was formed.

Fire decompoſed the acid of the lactate of potaſh.

Experiment 5 What was left in the retort was evaporated to dryneſs. This reſidue was brown, very acid, and when afterwards heated in a crucible of platina, ſwelled up and left a coal.

During this operation a ſmall quantity of ſulphuric acid was decompoſed.

The matter that remained in the retort was diſſolved in diſtilled water. The ſolution did not redden turnſole, but it gave a brown colour to paper tinged with curcuma. A little ſulphuric acid was added to ſaturation, and after evaporation cryſtals were obtained by cooling, which exhibited the characters of ſulphate of potaſh.

As the ſupernatant liquor had a metallic taſte, pruſſiate of lime was poured on it, which produced a bluiſh tinge.

Lactic acid decompoſed by fire leaves potaſh.

Experiment 6. The lactic acid may alſo be concentrated to dryneſs; heated to incineration in a crucible; afterwards diſſolved in a diſtilled water and precipitated by nitrate of ſilver. A muriate of ſilver is formed which may be ſeparated by the filter; and the fluid which contains the nitrate is then evaporated and decompoſed by heat. A white matter remains, which when diſſolved in water, affords with the tartareous acid an acidulous tartrite of potaſh.

Component parts of Scheele's lactic acid.

Hence I conclude that the lactic acid of Scheele is compoſed of acetic acid; muriate of potaſh; a ſmall portion of iron probably diſſolved in the acetic acid; and an animal matter.

X.

An Analyſis of the Magnetical Pyrites; with Remarks on ſome of the other Sulphurets of Iron. By CHARLES HATCHETT, *Eſq. F. R. S. From the Philoſophical Tranſactions for* 1804.

§ I.

OF the various metallic ſulphurets which conſtitute one of the grand diviſions of ores, none appear to be ſo univerſally diſperſed throughout the globe, as the ſulphuret of iron, commonly called Martial Pyrites; for the ſpecies and varieties of this are found at all depths, and in all climates and ſoils, whether ancient, or of alluvial and recent formation. It is remarkable alſo, that, under certain circumſtances, this ſulphuret is daily produced in the humid way; an inſtance of which, a few years back, I had the honour, in conjunction with Mr. Wiſeman, to lay before this Society; * and although, in regard to pecuniary value, the pyrites of iron may be conſidered as comparatively inſignificant, yet there is every reaſon to believe, that in the operations of nature, it is a ſubſtance of very conſiderable importance.

Univerſal diffuſion and importance of ſulphurets of iron.

§ II.

The ſpecies and varieties of martial pyrites, are in general ſo well known, and have been ſo frequently and accurately deſcribed, as to figure, luſtre, colour, and other external characters, that it would be totally ſuperfluous here to give any detailed account of them. One of the ſpecies, however, merits peculiar notice, as poſſeſſing the remarkable property of ſtrong magnetic polarity; and, although it has been deſcribed by modern mineralogiſts,† it does not appear to have been as yet ſubjected to any regular chemical examination; ſo that, whether it be a ſulphuret of iron inherently endowed with the magnetical property, or a ſulphuret in which particles of the ordinary magnetical iron ore are ſimply but minutely interſperſed, has to this time remained undecided.

Among the numerous ſpecies one is remarkable for ſtrong magnetic polarity.

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* Phil. Tranſ. for 1798, p. 567.

† Kirwan, Vol. II. p. 79. Widenmann, p. 792. Emmerling, 2d edit. Tom. II. p. 296. Karſten, p. 48. Brochant, Tome II. p. 232.

This

Its obvious or external properties.

This ſpecies is known by the name of Magnetical Pyrites, and is called by the Germans *Magnet-Kies*, or *Ferrum mineraliſatum magnetico-pyritaceum*.

It is moſt frequently of the colour of bronze, paſſing to a pale cupreous-red.

The luſtre is metallic.

The fracture is unequal, and commonly coarſe-grained, but ſometimes imperfectly conchoidal.

The fragments are amorphous.

The trace is yellowiſh-gray, with ſome metallic luſtre.

It is not very hard; but, when ſtruck with ſteel, ſparks are produced, although with ſome difficulty.

It is brittle, and is eaſily broken.

Where found.

This pyrites has been hitherto found only in ſome parts of Norway, Sileſia, Bavaria, and eſpecially at Geier, Meſſerſdorf, and Breitenbrunn in Saxony; but, having received ſome ſpecimens from the Right Hon. Charles Greville, F. R. S. I was ſtruck with their reſemblance to the pyrites of Breitenbrunn, which happened at that time to be in my poſſeſſion; and, upon trial, I found that they were magnetical, and agreed with the latter in every particular. Their magnetic power was ſuch as ſtrongly to affect a well-poized needle, of about three inches in length; a piece of the pyrites, nearly two inches ſquare, acted upon the needle at the diſtance of four inches.

Degrees of magnetiſm.

The powder (which is blackiſh-gray, with but little metallic luſtre) is immediately taken up by a common magnet; but the pyrites does not act thus on the powder, nor on iron filings, unleſs it has been placed for ſome time between magnetical bars; then indeed it acts powerfully, turns the needle completely round, attracts and takes up iron filings, and ſeems permanently to retain this addition to its original power.

In the ſpecimens which I obtained, the north pole was generally the ſtrongeſt.

This pyrites was found in Wales, about the year 1798, by the Hon. Robert Greville, F. R. S. who ſent the ſpecimens above deſcribed to his brother, the Right Hon. C. Greville, with the following account.

Abundantly found in Caernarvonſhire.

" It is found in great abundance in Caernarvonſhire, near " the baſe of the mountain called Moel Elion, or probably with " more accuracy Moel Ælia, and oppoſite to the mountain

" called Mynydd Mawr. Theſe mountains form the entrance " into a little cloſe valley, which leads to Cywellin lake, near " Snowdon, a little beyond the hamlet of Bettws.

" The vein appears to be ſome yards in depth and breadth, " and ſeems to run from north to ſouth, as it is found on " Mynydd Mawr, which is acroſs the narrow valley, and " oppoſite to Moel Ælia."

Mr. R. Greville, in another part of his letter, ſtates that copper ore has been worked in ſeveral of the adjacent places, and that, many years ago, Capt. Williams, of Glan yr Avon, employed ſome miners at the place where this pyrites is found, but the undertaking proved unproductive. Yellow copper ore is certainly in the vicinity; for ſome portions of it were adhering to the ſpecimens which have been mentioned; and I ſhall here obſerve, that the ſtone which accompanies the magnetical pyrites, is a variety of the lapis ollaris or pot-ſtone, of a pale grayiſh-green, containing ſmooth cubic cryſtals of common pyrites.

§ III.

Magnetic pyrites is ſlightly oxidable by the weather.

From the appearance of thoſe parts of the magnetical pyrites which have been expoſed to the weather, it ſeems to be liable to oxidizement, but not to vitriolization.

Sp. gravity.

The ſpecific gravity, at temperature 65° of Fahrenheit, is 4518.

Effect of mere heat by blowpipe:

When expoſed to the blowpipe, it emits a ſulphureous odour, and melts into a globule nearly black, which is attracted by the magnet.

—in a crucible;

Five hundred grains, in coarſe powder, were expoſed, in a ſmall earthen retort, to a red heat, during three hours. By this operation, the weight of the powder was very little diminiſhed; neither was there any appearance of ſulphur in the receiver, which however ſmelt ſtrongly of ſulphureous acid.

—in the muffle.

Five hundred grains of the ſame were put into a flat porcelain crucible, which was kept in a red heat, under a muffle, during four hours. The powder then appeared of a dark gray, with a tinge of deep red, and weighed 432.50 grains. The loſs was therefore 67.50 = 13.50 *per cent.* but, upon examining the reſiduum, I found that only part of the ſulphur had been thus ſeparated.

The

Partly ſoluble in dilute ſulph. acid.

The magnetical pyrites, when digeſted in dilute ſulphuric acid, is partially diſſolved, with little efferveſcence, although there is a very perceptible odour of ſulphuretted hydrogen.

The ſolution is of a very pale green colour.

Precipitates.

Pure ammonia produced a dark green precipitate, tending to black; and pruſſiate of potaſh formed a very pale blue precipitate, or rather a white precipitate mingled with a ſmall portion of blue. The whole of the latter, however, by expoſure to the air, gradually aſſumed the uſual intenſity of Pruſſian blue; and the blackiſh green precipitate, formed by ammonia, became gradually ochraceous. Theſe effects therefore fully prove, that the iron in the ſolution was, for the greater part, at the minimum of oxidizement, ſo as to form the green ſulphate, and white pruſſiate, of iron; * and, conſequently, that the iron of the magnetical pyrites is either quite, or very nearly, in the ſtate of perfect metal.

Effect of nitric acid.

This pyrites, when treated with nitric acid, of the ſpecific gravity of 1.38, diluted with an equal quantity of water, is at firſt but little affected; but, when heat is applied, it is diſſolved, with much efferveſcence, and diſcharge of nitrous gas; the efferveſcence, however, is by no means ſo violent as when the common pyrites are treated in a ſimilar manner. It is alſo worthy of notice, that if the digeſtion be not of too long duration, a conſiderable quantity of ſulphur, *in ſubſtance*, is ſeparated; whilſt, on the contrary, ſcarcely any can be obtained from the common pyrites, when treated in a ſimilar manner; although I ſhall ſoon have occaſion to prove, that the real quantity of ſulphur is much more conſiderable in the latter than in the former.

Of muriatic acid.

As ſoon as muriatic acid is poured on the powder of the magnetical pyrites, a ſlight efferveſcence is produced, which becomes violently increaſed by the application of heat; a quantity of gas is diſcharged, which, by its odour, by its inflammability, by the colour of the flame, by the depoſition of ſulphur when burned, and by other properties, was proved to be ſulphuretted hydrogen.

During the digeſtion, ſulphur was depoſited, which ſo enveloped a ſmall part of the pyrites, as to protect it from the farther action of the acid.

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* Récherches ſur le Bleu de Pruſſe, par M. Prouſt. Annales de Chimie, Tome XXIII. p. 85.

Muriatic ſolution.

The ſolution was of a pale yellowiſh-green colour. With pruſſiate of potaſh it afforded a pale blue precipitate, or rather a white precipitate mixed with blue; and with ammonia it formed a dark blackiſh-green precipitate, which gradually became ochraceous; ſo that theſe effects corroborated the concluſions which were founded on the properties of the ſulphuric ſolution, namely, that the iron contained in the pyrites, is almoſt, if not quite, in the metallic ſtate.

Other experiments were made; but, as they merely confirm the above obſervations, I ſhall proceed to give an account of the analyſis.

§ IV.

ANALYSIS OF THE MAGNETICAL PYRITES.

Analyſis. 100 gr. magnetic pyrites were digeſted in muriatic acid. Partial ſolution. Refidue treated with nitro-mur. acid. Some ſulphur ſeparated.

A. One hundred grains, reduced to a fine powder, were digeſted with two ounces of muriatic acid, in a glaſs matraſs placed in a ſand bath. The effects already deſcribed took place; and a pale yellowiſh-green ſolution was formed. The reſiduum was then again digeſted with two parts of muriatic acid mixed with one of nitric acid; and a quantity of pure ſulphur was obtained, which, being dried, weighed 14 grains.

To the acid ſolutions was added ſome nitric acid and the iron was then precip. by ammonia. The precip. was boiled with potaſh and then reduced to black oxide.

B. The acid in which the reſiduum had been digeſted, was added to the firſt muriatic ſolution; ſome nitric acid was alſo poured in, to promote the oxidizement of the iron, and thereby to facilitate the precipitation of it by ammonia, which was added after the liquor had been boiled for a conſiderable time. The precipitate thus obtained was boiled with lixivium of potaſh; it was then edulcorated, dried, made red-hot with wax in a covered porcelain crucible, was completely taken up by a magnet, and, being weighed, amounted to 80 grains.

The potaſh lixivium gave no alumina.

C. The lixivium of potaſh was examined by muriate of ammonia, but no alumina was obtained.

The firſt ſolution liquor was precip. by muriate of barytes. The ſulphate of barytes thus obtained ſhewed the quantity of ſulph. acid.

D. To the filtrated liquor from which the iron had been precipitated by ammonia, muriate of barytes was added, until it ceaſed to produce any precipitate; this was then digeſted with ſome very dilute muriatic acid, was collected, waſhed, and, after expoſure to a low red heat for a few minutes in a crucible of platina, weighed 155 grains. If therefore the quantity of ſulphur, converted into ſulphuric acid by the preceding operations, and precipitated by barytes, be calculated according to the accurate experiments of Mr. Chenevix, theſe 155 grains of ſulphate of barytes will denote, nearly, 22.50 of ſulphur; ſo that, with the addition of the 14 grains previouſly obtained in ſubſtance, the total quantity will amount to 36.50.

Eſtimate of the pure iron in the oxide.

E. Moreover, from what has been ſtated it appears, that the iron which was obtained in the form of black oxide, weighed 80 grains; and, by adding theſe 80 grains to the 36.50 of ſulphur, an increaſe of weight is found = 16.50. This was evidently owing to the oxidizement of the iron, which, in the magnetical pyrites, exiſts quite, or very nearly, in the metallic ſtate, but, by the operations of the analyſis, had received this addition. The real quantity of iron muſt, on this account, be eſtimated at 63.50.

Component parts. Sulphur 4. and iron 7.

One hundred grains, therefore, of the magnetical pyrites, yielded,

$$\begin{array}{lcl} \text{Sulphur}\left\{\begin{matrix}\text{A. } 14 \\ \text{D. } 22.50\end{matrix}\right\} & & 36.50 \text{ grains.} \\ \text{Iron} \quad \text{E.} & = & 63.50 \\ \hline & & 100. \end{array}$$

Analyſis repeated by acidifying all the ſulphur by nitric acid. Same reſult.

This analyſis was repeated in a ſimilar manner, excepting that the whole was digeſted in nitric acid, until the ſulphur was intirely converted into ſulphuric acid. To the liquor which remained after the ſeparation of the iron by ammonia, muriate of barytes was added, as before, and formed a precipitate which weighed 245 grains. Now, as the ſulphuric acid in ſulphate of barytes is eſtimated by Mr. Chenevix at 23.5 *per cent.* and the ſulphur which is required to form the ſulphuric acid contained in 100 parts of ſulphate of barytes, at 14.5 *, it follows, that 245 grains of dry ſulphate of barytes, contain ſulphuric acid equal, very nearly, to 36 grains of ſulphur; ſo that the two analyſes corroborate each other. The proportion of ſulphur in the magnetical pyrites, may therefore be ſtated at 36.50, or indeed at 37 *per cent.* if ſome ſmall allowance be made for the occaſional preſence of earthy particles; a minute portion of quartz having been found, by the laſt analyſis, after the complete acidification of ſulphur.

The iron in this and in all pyrites, is very nearly in the metallic ſtate.

The increaſe produced, by the operations of the analyſis, in the weight of the iron, aroſe, as I have already remarked, from the addition of oxygen; for the iron, as obtained by the analyſis, was in the ſtate of black oxide; but in this, and indeed in all pyrites, it undoubtedly exiſts very nearly, or quite, in the ſtate of perfect metal. Now the black oxide of iron, called Protoxide by Dr. Thomſon *, has been proved, by Lavoiſier and Prouſt, to conſiſt of 100 parts of metallic iron combined with 37 of oxygen, thus forming 137 of black oxide; the exact proportion of oxygen is therefore 27 *per cent*, and 80 grains of this oxide muſt contain 21.6 of oxygen. But, in the above analyſes of the magnetical pyrites, the increaſe of weight did not amount to more than 16.5; and we may therefore conclude that, in all probability, a quantity of oxygen = 5.1 was previouſly combined with ſome part, or with the general maſs, of the iron in the pyrites. A ſmall part of the abovementioned increaſe of weight, muſt likewiſe have ariſen from another cauſe; for, although the true proportions of the black oxide of iron are 27 of oxygen and 73 of iron, (ſo that 100 parts of the latter abſorb 37 of the former,) yet, in actual practice, it is difficult to obtain it exactly in this ſtate, and there is commonly a ſmall exceſs of weight: this I have repeatedly obſerved, in many experiments, ſome of which were purpoſely made. When, for inſtance, 100 parts of fine iron wire were diſſolved in muriatic acid, and afterwards precipitated by ammonia, edulcorated, dried, and made red-hot with a ſmall quantity of wax in a covered porcelain crucible, the weight, inſtead of 137, uſually amounted to 139 or 140. The quantity of wax employed, certainly did not afford a ponderable quantity of coal, or other reſiduum; but the real cauſe of the increaſe of weight, appears to be the air, which can ſcarcely be completely excluded, and which, after the wax is burned, combines with the ſuperficial part of the oxide, and converts a portion of it into the red or peroxide; ſo that the ſurface in the crucible appears brown, when compared with the interior.

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* Tranſactions of the Royal Iriſh Academy, Vol. VIII. p. 240.

To this cauſe, therefore, I am inclined alſo to attribute a ſmall part of the increaſe obſerved in the weight of the iron obtained by the preceding analyſes.

§ V.

Comparative analyſis of other pyrites, by acidifying the whole of the ſulphur.

Before I make any obſervations on the nature of the ſulphuret which has been proved to conſtitute the magnetical pyrites, it may be proper to ſtate ſome comparative analyſes which I have made of ſeveral of the common pyrites; and, as the method employed was preciſely the ſame as that which has been deſcribed, all that ſeems to be requiſite, is to give an account of the reſults.

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* Syſtem of Chemiſtry, 2d edition, Vol. I. p. 147.

In each analyſis, the whole of the ſulphur was converted into ſulphuric acid, which was precipitated by barytes; and, in the ſelection of the ſpecimens, great attention was paid to take the internal parts of the fragments, and not to make uſe of any which exhibited an appearance of decompoſition, or of extraneous ſubſtances.

The iron was, as before, reduced to the ſtate of black oxide; and the addition of weight in each ſeparate analyſis, correſponded, within a few fractional parts, with the proportion of oxigen requiſite to form into black oxide a given quantity of metallic iron, equal to that which in each pyrites was aſcertained to be the real proportion, by deducting the quantity of ſulphur from the total quantity of each pyrites.

The iron, therefore, in theſe is completely metallic, and as ſuch is ſtated in the following reſults:

The component parts of the common pyrites were, at a medium, 9 ſulphur and 8 iron, with no great variation.

No. 1. Pyrites in the form of dodecaedrons with pentagonal faces. - - Specific gravity 4830.	Sulphur	52.15
	Iron	47.85
		100.
No. 2. Pyrites in the form of ſtriated cubes.	Sulphur	52.50
	Iron	47.50
		100.
No. 3. Pyrites in the form of ſmooth poliſhed cubes, found in the lapis ollaris which accompanies the magnetical pyrites. Specific gravity 4831.	Sulphur	52.70
	Iron	47.30
		100.
No. 4. Radiated pyrites. - - Specific gravity 4698.	Sulphur	53.60
	Iron	46.40
		100.
No. 5. A ſmaller variety of radiated pyrites. Specific gravity 4775.	Sulphur	54.34
	Iron	45.66
		100.

Conſidering the difference in the figure, luſtre, and colour of theſe pyrites, I expected to have found a much greater difference in the proportions of their component ingredients; but, as the reſults are the average of ſeveral experiments, I have not any reaſon to doubt their accuracy.

Regular cryſtals contain rather more iron than other pyrites.

The pyrites cryſtallized in regular figures, ſuch as cubes and dodecaedrons, according to the above analyſes, contain leſs ſulphur, and more iron, than the radiated pyrites, and perhaps than others which are not regularly cryſtallized. This difference, however, is not conſiderable; for the dodecaedral pyrites, which afforded the ſmalleſt quantity of ſulphur of any of the regularly cryſtallized pyrites, yielded 52.15; and the radiated pyrites, No. 5, gave 54.34; the difference, therefore, is only 2.19. So that the mean proportion of ſulphur, in all the pyrites which were examined, is 53.24 *per cent.* and, taking the proportion of ſulphur in the magnetical pyrites at 36.50 or 37, the difference between this and the mean of the common pyrites will be 16.74 or 16.24. The magnetical pyrites, therefore, is quite diſtinct, as a ſulphuret of iron, from the common martial pyrites; and, in the following obſervations I ſhall prove, that a ſulphuret conſiſting of the proportions laſt mentioned, has till now been unknown as a product of nature.

§ VI.

Hiſtory of pyrites.

Although pyrites is one of the moſt common of mineral ſubſtances, yet the diſcovery of its real nature is comparatively of a late date; for it appears that even Agricola (whoſe knowledge of mineral bodies was certainly great, conſidering the ſtate of ſcience in his time) was not acquainted with its characteriſtic ingredient, namely, *iron*. According to Henckel, this was firſt noticed by our countryman Martin Liſter, a member of this learned Society, who ſays, "*Pyrites purus putus ferri metallum eſt.*"

Experiments of Prouſt.

Two ſulphurets of iron: artificial, which is eaſily ſoluble in acids, and natural, leſs ſoluble.

From the time of Henckel, pyrites ſeems little to have attracted the notice of chemiſts, until Mr. Prouſt, the learned profeſſor of chemiſtry at Madrid, publiſhed two memoirs, in which he ſtates, that there are two ſulphurets of iron, the one being artificial, and the other natural. The firſt is the ſulphuret which is formed in laboratories, by adding ſulphur to red-hot iron, or by expoſing both of them to heat in a retort. This is diſtinguiſhed from the ſecond ſulphuret, (which is the common martial pyrites,) by its eaſy ſolubility in acids, eſpecially in muriatic acid, by the formation of ſulphuretted hidrogen gas during the ſolution of the ſulphuret in the laſt named acid, by its colour, and by its inferior denſity.

Artificial ſulphuret, 6 ſulphur and 10 iron; natural, 9 ſulphur and 10 iron.

According to Mr. Prouſt, the firſt or artificial ſulphuret is compoſed of 60 parts of ſulphur, combined with 100 parts of iron; whilſt the ſecond ſulphuret, or common pyrites, conſiſts of 90 parts of ſulphur and 100 of iron.

The exceſs of ſulphur is eaſily ſeparated from pyrites; but not the other portion.

He moreover obſerves, that the ſulphur of the firſt ſulphuret is difficultly ſeparated; but that the exceſs which is in the ſecond ſulphuret, or common pyrites, is eaſily expelled, and is that portion which is obtained by diſtillation, the reſiduum being then reduced to the ſtate of the firſt ſulphuret [*]. 100 parts, therefore, of this ſubſtance, are compoſed of 62.50 of iron and 37.50 of ſulphur; and 100 parts of common pyrites are, according to this ſtatement, compoſed of 52.64 of iron and 47.36 of ſulphur.

They are the maxima and minima of the ſulphurets.

Theſe proportions Mr. Prouſt conſiders as the minimum and maximum of the ſulphurets of iron. For the latter he allows ſome variation; but the compoſition of the former he regards as fixed by the invariable law of proportions [†]; although he obſerves, that *it has not as yet been diſcovered in the mineral kingdom* [‡].

In ſupport of theſe aſſertions, Mr. Prouſt ſtates,

Experiments of Prouſt on this ſubject.

1. That the pyrites found near Soria, when diſtilled in a retort heated to redneſs, afforded nearly 20 *per cent.* of ſulphur.

2. That the reſiduum of the above diſtillation had loſt the external characters and chemical properties of pyrites, and had aſſumed thoſe of the artificial ſulphuret of iron.

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[*] *Journal de Phyſique*, Tome LIII. p. 89, and Tome LIV. p. 89. From pp. 91 and 92 of Tome LIV. it is evident, that the author does not mean to aſſert, that the firſt ſulphuret contains 60 *per cent.* of ſulphur; but that 100 parts of iron are combined with 60 of ſulphur, and form 160 of the ſulphuret. In like manner, when 90 of ſulphur are united with 100 of iron, a ſubſtance analogous to common pyrites is formed, which weighs 190 grains or parts.

See alſo our Journal, I. 109, 269, 253, for tranſlations of theſe excellent memoirs.

[†] *Journal de Phyſique*, Tome LIII. p. 90.

[‡] " La regne minéral, juſqu'ici, ne nous a point encore préſenté le fer ſulphuré au minimum." *Journal de Phyſique*, Tome LIV. p. 93.

3. That when to this reſiduum a quantity of ſulphur was added, and the whole was diſtilled in a degree of heat not too great, the 20 *per cent.* of ſulphur, which had been ſeparated by the firſt diſtillation, was, by this, again reſtored; and the maſs in the retort thus recovered nearly the original colour, luſtre, and chemical properties of the pyrites.

4. That, by adding ſulphur to iron filings, or fine iron wire, heated to a low red in a retort, a compound is obtained, in which the proportion of ſulphur amounts only to about 20 or 30 parts; but, if this compound is again treated with ſulphur in a red heat, a ſulphuret is formed, which is readily diſſolved in acids, and plentifully affords ſulphuretted hydrogen gas.

This is the real minimum of the ſulphurets of iron, fixed by the invariable law of proportions, (according to Mr. Prouſt,) at 59 or 60 of ſulphur and 100 of iron, the former being (as I have already obſerved) in the proportion of 37.50 *per cent.*

5, and laſtly. That when this ſulphuret is again mixed and diſtilled with ſulphur, (due attention being paid to the degree of heat,) the product is found to have aſſumed moſt of the chemical and external properties of the natural common pyrites, denſity alone being excepted.

The magnetical pyrites is that which Prouſt conſidered as having been only produced by art.

The application of the above obſervations, to the principal ſubject of the preſent paper, is ſufficiently obvious; for, when it is conſidered, that the magnetical pyrites is ſo different from the common pyrites, in colour, hardneſs, ſolubility in ſulphuric acid, and more eſpecially in muriatic acid, with the copious production of ſulphuretted hydrogen gas; when, by analyſis, it has been found to conſiſt of 36 or 37 of ſulphur, combined with about 63 of metallic iron; and, when the artificial ſulphuret of iron which has been lately deſcribed, is proved to agree with the magnetical pyrites in the nature and proportions of its component ingredients, and in every one of the above-mentioned properties; it is evident that the magnetical pyrites is identically the ſame with this ſulphuret, which hitherto has remained undiſcovered in nature, and has only been known as a product of our laboratories. In order however more fully to ſatiſfy myſelf, I made experiments on the artificial ſulphuret, which I formed with ſulphur and fine iron wire.

The artificial pyrites agrees with the natural magnetic pyrites.

This ſubſtance agreed, in all the properties which have been noticed, with the magnetical pyrites; and the precipitates obtained by adding pruſſiate of potaſh, and ammonia, to the muriatic and ſulphuric ſolutions, were preciſely ſimilar. The ſpecific gravity was 4390, whilſt (as I have already remarked) that of the magnetical pyrites is 4518.

(To be concluded.)

XI.

Anſwer to a Letter of C. L. *with other Remarks, on the Images formed by convex Lenſes. In a Letter from Mr.* Ezekiel Walker.

To Mr. NICHOLSON.

Dear Sir,

Remarks on the Letters of C. L.

C. L. in his letter in your laſt Number, repreſents my firſt paper as deficient in "philoſophical correctneſs," and wanting "the calm ſpirit of philoſophy." But in attempting to prove my errors, he has proved nothing but his own want of knowledge: and to thoſe who wiſh to ſee an example of C. L.'s "*calm ſpirit of philoſophy*," I beg leave to recommend the peruſal of his letter in Vol. IX. p. 235, of this Journal.—Theſe ſtrange aſſertions are followed by another ſtill more ſtrange.

Quotation, in which C. L. denies Mr. W.'s facts.

He ſays, "*I will beg leave* to wave any attention to the hiſtory of Mr. Walker's experiments upon which I have animadverted, and *to deny his facts*."

Thinking that I did not underſtand his meaning correctly, I examined the ſide-note, which runs thus: "C. L. declines any further diſcuſſion reſpecting Mr. Walker's experiments with lenſes, *but denies the facts*."

Mr. W.'s reply.

As there is no reaſoning with a man who will deny facts, I ſhall decline examining the remainder of C. L.'s letter.

Your other anonymous correſpondent, who has denied the truth of my experiments, will be more eaſily anſwered.

Horizontal moon.

The opinions which have been advanced reſpecting the horizontal luminaries, ſince the days of Ptolomy, who was born about the year 70 of the Chriſtian æra, are ſo numerous, that it is ſcarcely poſſible to form a new one. This correſpondent has picked up the theory of Des Cartes, which was confuted more than 100 years ago by Mr. Molyneux. See Philoſ. Tranſ. abr. Vol. I. p. 221; or Dr. Hutton's Mathematical Dictionary, Vol. II. p. 74. I ſhall paſs over this writer's optical errors by juſt obſerving, that he has been doing nothing leſs than denying an eſtabliſhed property in optics.

Farther remarks on the image formed by a lens,

But before I proceed, it may be convenient to thoſe who are not in poſſeſſion of your two Numbers which contain my former papers on this ſubject, to know what that property in optics is, which has met with ſo much oppoſition.

aſſerted to vary with the aperture.

I have proved experimentally, that the image of a candle in the focus of a double convex lens, decreaſes in magnitude as the aperture of the lens is contracted*: And I have alſo advanced, that the image of the ſun or moon, in the focus of the object-glaſs of a teleſcope, decreaſes with its aperture †.

This poſition aſcribed to Newton,

Now it is well known to thoſe who underſtand optics, that this property, the truth of which has been denied ſo poſitively, was diſcovered by Sir Iſaac Newton: and a demonſtration of it may be ſeen in the Optics of Newton, Emerſon, Smith, and Martin, and alſo in the Encyclopædia Britannica, &c.

becauſe he notes the ſpace of diffuſion in pencils of rays.

"The breadth of the leaſt circular ſpace into which object-glaſſes of teleſcopes can collect all ſorts of parallel rays, is about the 27½ part of half the aperture of the glaſs, or 55th part of the whole aperture." (Newton's Optics, 2d Edit. p. 73). Hence it is evident, that if you contract the breadth of the object-glaſs from 10 to 1, the breadth of the circle of light, in its focus, will be contracted in the ſame proportion.

Whence it is deduced, that the image alſo will vary.

In my next paper, this intereſting property of light will be further explained.

I am, Dear Sir,
Your humble ſervant,
EZ. WALKER.

Lynn, March 16, 1805.

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[*] Philoſophical Journal, Vol. IX. p. 164.

[†] Philoſophical Journal, Vol. X. p. 110.

XII.

Account of an Aerostatic Voyage, made by M. GAY-LUSSAC *on the 29th Fructidor, in the Year 12. Read to the National Institute, on the 9th Vendemiaire, in the Year 13.*[*]

Motives of the ascent.

IN the account of the first aeroſtatic voyage which M. Biot and I had the honour to ſubmit to the Inſtitute, we announced that, from the ſurface of the earth to the height of 3977 metres (13024.675 feet), the magnetic power did not experience any ſenſible diminution; and at the ſame time we ſignified a deſire to undertake new aſcents, to aſcertain this important fact at greater heights. We ſoon found that this was alſo the wiſh of many members of the Inſtitute; and, encouraged by the general intereſt which our firſt voyage had excited, we reſolved on making a ſecond very ſhortly; but our aeroſtat not being capable of carrying us together to a greater height than in our firſt aſcent, it was agreed between us that I ſhould go alone. From this moment all our attention was given to the inſtruments which it was requiſite for me to carry; and their conſtruction, which was again entruſted to M. Fortin, as well as an operation which the balloon underwent to give it more levity, retarded my departure till the 29th of laſt Fructidor.

Conſtruction of the inſtruments.

Inſtructed by the experience of our former aſcent, we made ſome changes in our inſtruments; and, in the firſt place,

Horizontal needle.

that the oſcillations of the horizontal needle might be leſs affected by the rotation of the balloon, we cauſed a new one to be conſtructed, only 15 centimetres in length. Thus, its oſcillations being more rapid than thoſe of the balloon, it would be eaſier to determine their duration.

Dipping needle.

We made greater modifications in the dipping needle. That we might not be obliged, at each obſervation, to replace it in the magnetic meridian, and to adjuſt its axis truly horizontal, we ſuſpended the metallic chape which ſupports it by a ſilken thread; and, to judge of its inclination, we fixed a portion of a tranſparent circle, graduated in diviſions, to the chape. The whole apparatus was very light, the tenſion on the ſilk thread but trifling, and the needle could reſume its meridian with facility. M. Coulomb, after having touched the needle, proved it by the method which he propoſed in the Memoirs of the Inſtitute; and he found that it gave an inclination of 70.5° of the common ſcale. In one of its poſitions, which was that in which it was to remain, it varied 31°.

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[*] From Annales de Chimie, No. 154, Fructidor, An. XIII.

Azimuth compaſs.

In our firſt aſcent, the under ſurface of the glaſs which covered our azimuth-compaſs was covered with water, and we were prevented from ſeeing the ſhadow of a horizontal thread, which ſerved us for a ſtyle. To avoid this inconvenience, it was ſufficient to remove the glaſs of the compaſs; in other reſpects, no alteration was made in its firſt diſpoſition. M. Lepine again ſupplied us with two ſecond-watches, one of which was a ſtop-watch: it was with the latter that I made all my obſervations.

Thermometers.

The thermometer I uſed, was the centigrade mercurial thermometer. To guard it from the action of the ſun, we placed it within two concentric cylinders of paſteboard covered with gilt paper, one of which was about four centimetres in diameter, and the other ſix. Our hygrometers with four hairs, of the conſtruction of M. Richer, were diſpoſed in nearly a ſimilar manner. The two glaſs balloons in which I was to bring the air, were exhauſted to nearly a millimetre of mercury, and we were ſatisfied by leaving them in this ſtate for eight days, that they perfectly retained the vacuum. For fear of an accident, we had provided a third balloon of braſs; but very fortunately it was uſeleſs.

Hygrometers.

Barometers.

Our two barometers have not a conſtant level; and to get the true barometric heights, we formed a table of compariſon, by placing them under the receiver of the pneumatic machine, and meaſuring their depreſſion from five to five centimetres, by means of a ſtandard, the reſervoir of which has a conſtant level, and which is provided with a very good ſcale. Being no longer obliged to obſerve more than the upper level to get the true barometric heights, the number of obſervations was diminiſhed to one half; which is of great importance when the attention is divided between perſonal ſafety and delicate experiments.

The apparatus for meaſuring the electricity of the air rendered uſeleſs.

Such nearly were the eſſential inſtruments I took with me in my voyage. I was alſo provided with an apparatus for determining the electricity of the air; but a few moments after I quitted the earth, I loſt the two metallic wires which were to attract the electricity at 50 and at 100 metres below me, and I could not make any uſe of it. It will be ſeen that we had avoided every thing which could influence our needles; and even our anchor, although ſuſpended 50 metres below the boat, was of wood armed with copper.

This is not the place to mention the precautions which M. Conté had taken, that this new aſcent might be free from danger; but it is to be wiſhed that he would himſelf publiſh what a long and enlightened experience has taught him on this ſubject. For ourſelves, we owe him a great tribute of gratitude for his trouble and the intereſt he has taken in our voyages; and we are bound to acknowledge, that if they have been ſo fortunate, we are indebted for it to his provident cares.

Departure.

State of the atmoſphere on the earth.

All our inſtruments being ready, my departure was fixed for the 29th Fructidor: In fact, on that day I aſcended from the Conſervatory of the Arts and Manufactures, at 9^h 40′, the barometer being at 76.525 centimetres, the hygrometer at 57.5°, and the thermometer at 27.75°. M. Bouvard, who makes meteorological obſervations daily at the Obſervatory of Paris, judged the atmoſphere to be greatly charged with vapour, but without clouds. In fact, I had ſcarcely riſen 1000 metres when I perceived a light vapour diffuſed through all the atmoſphere, which rendered the ſight of remote objects confuſed.

Firſt experiment on the oſcillations of the horizontal needle.

Arrived at the height of 3032 metres (9929.8 F.), I began to make the horizontal needle oſcillate, and this time I obtained 20 oſcillations in 83″, while on the earth, and in other reſpects in the ſame circumſtances, it required 84.33″ to make the ſame number *. Although my balloon was affected with the rotatory motion we had perceived in our firſt experiment, the rapidity of the motion of our needle permitted me to count 20, 30, and even 40 oſcillations.

Inclination of the needle at a height of 12651.365 feet, the ſame as on the earth.

At the height of 3863 metres (12651.365 F.), I found the variation of my needle, taking the mean of the amplitude of its oſcillations, was ſenſibly 31°, as on the earth. It re-

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* Although I have here indicated hundredth parts of a ſecond, I am aware that I cannot obſerve ſuch ſmall fractions; but I obtained them by diviſions, becauſe on the earth I generally made 30 oſcillations, which required 126.5″.

quired much time and patience to make this obſervation; becauſe, although borne by the maſs of the atmoſphere, I perceived a ſlight wind which continually deranged the compaſs, and after ſeveral unſucceſsful attempts, I was obliged to give up making new ones. Nevertheleſs, I believe the obſervation I have ſtated above is entitled to ſome confidence.

The obſervations with the azimuth compaſs not to be depended upon.

Some time after I wiſhed to obſerve the dipping-needle; but behold what had happened. The drought, favoured by the action of the ſun in a rarefied air, was ſuch, that the compaſs was ſo much warped as to have bent the metallic circle on which the diviſions were traced, and to be itſelf crooked. The motions of the needle therefore had not the ſame freedom; but independently of this diſappointment, I had obſerved that it was very difficult to obſerve the dip of the needle with this apparatus. In ſhort, when I placed the compaſs ſo as to make the ſhadow of the horizontal thread, which ſerved as a ſtyle, coincide with a fixed line, the motion I had given to the compaſs communicated one to the needle, and by the time this was nearly reſtored to reſt, the ſhadow of the ſtyle no longer coincided with the fixed line. It was neceſſary to place the compaſs again in a horizontal poſition, and during the time which this operation required, every thing was again deranged. Unwilling to perſiſt in making obſervations on which I could place no reliance, I abandoned them entirely; and diveſted of every other care, I gave all my attention to the oſcillations of the horizontal needle. I am nevertheleſs convinced, by the knowledge of the defects of our compaſs, that it is poſſible to employ one more ſuitable, which would determine the dip with great preciſion. In trying this experiment, I lowered the other needles ſeparately in linen bags, to 15 metres below the boat.

Cauſes of its defects.

The experiment ſtill practicable.

The aggregate of the reſults formed into a table.

Calculation of the heights by the barometer compared with one on the earth:

That the aggregate of all the reſults which I obtained may be more eaſily ſeen, I have collected them into a table which is at the end of this memoir; and they are placed there as they occurred to me, with the correſponding indications of the barometer, thermometer, and hygrometer. The heights have been calculated, according to the formula of M. Laplace, by M. Gouilly, engineer of bridges and highways, who has been ſo kind as to take this trouble. The barometer not having varied ſenſibly on the day of my aſcent, from ten o'clock to three, to calculate the different heights at which I made

made my obſervations, the height of the barometer on the earth has been taken at 76.568 centimetres, which was the caſe at three o'clock; a height which, conformably to the obſervations made by M. Bouvard at the Obſervatory, is greater by 0.43 millimetres than that which had been obſerved at the moment of my departure. The heights of the barometer in the atmoſphere have been brought to thoſe which would have been indicated by a barometer with an uniform level placed in the ſame circumſtances, and the mean between the obſervations of the two barometers has been taken for each height. The temperature on the earth having alſo varied very little between ten and three o'clock, it has been ſuppoſed conſtant and equal to 30.75° of the centigrade thermometer.

and of the temperature.

The irregular variation of the temperature ariſes from the change of place during the obſervation.

Now on looking into the table it will be evident, in the firſt place, that the temperature follows an irregular courſe with reſpect to correſponding heights; which, no doubt, is occaſioned by the obſervations having been made ſometimes in aſcending, and ſometimes in deſcending, and the thermometer having followed theſe variations too ſlowly. But if the degrees of the thermometer alone are conſidered, which form with each other a continually decreaſing ſeries, a more regular law will be found. Thus the temperature on the earth being 27.75°, and 8.5° at the height of 3691 metres, if the difference of the heights is divided by that of the temperatures, we ſhall at once obtain 191.7 metres (627.8175 feet) of elevation for each degree of the reduction of the temperature. By performing the ſame operation for the temperatures 5.25° and 0.5°, as well as for thoſe of 0° and −9.5°, we ſhall find, in both caſes, 141.6 metres (463.74 feet) of elevation for each degree of the reduction of the temperature: which ſeems to indicate that near the ſurface of the earth the heat is governed by a law which decreaſes leſs than at an elevation in the atmoſphere, and that at length it follows a decreaſing arithmetical progreſſion. If it be ſuppoſed that from the ſurface of the earth, where the thermometer was at 30.75° to the height of 6977 metres (22849.675 feet), where it had fallen to −9.5°, the heat had diminiſhed as the elevations increaſed, an elevation of 173.3 metres (567.557 feet) will correſpond to each degree of the reduction of the temperature.

Difference of elevation correſponding to a degree of the difference of temperature.

Unſteady courſe of the hygrometer.

The courſe of the hygrometer was very ſingular. At the ſurface of the earth it was only at 57.5°, while at the height of 3032 metres it marked 62° : from this point it was continually falling to the height of 5267 metres, where it indicated no more than 27.5° ; and from thence to the height of 6884 metres, it roſe gradually to 34.5°. If, by theſe reſults, it is wiſhed to determine the law of the quantity of water diſſolved in the air at different elevations, it is clear that attention muſt be paid to the temperature ; and on joining this conſideration with it, it will be ſeen that it follows an extremely decreaſing progreſſion.

Magnetic oſcillations at different heights.

If now the magnetic oſcillations are conſidered, it will be obſerved that the time for ten oſcillations, made at different heights, is ſometimes above and ſometimes below 42.16″, which they require upon the earth. By taking the mean of all the oſcillations made in the atmoſphere, ten oſcillations would require 42.20″, a quantity which differs very little from the preceding ; but on conſidering only the laſt obſervations which were made at the greateſt heights, the time for ten oſcillations would be a little leſs than 42.16°, which would indicate, on the contrary, that the magnetic power had been a little augmented. Without meaning to draw any conſequence from this apparent ſlight increaſe, which may very probably ariſe from the errors to which experiments of this deſcription are liable, I conclude that the total of the reſults I now offer, confirms and extends the fact which M. Biot and myſelf had obſerved, and which, as well as the univerſal gravitation, proves that the magnetic power does not experience ſenſible variations at the greateſt heights to which we can aſcend.

The magnetic power is not ſenſibly altered at the greateſt elevations.

Probable objections to this concluſion refuted.

The conſequence we have drawn from our experiments, will appear a little too precipitate to thoſe who recollect that we were unable to make experiments on the inclination of the touched needle. But if it be obſerved that the power which cauſes an horizontal needle to oſcillate, is neceſſarily dependent upon the intenſity and direction of the magnetic power itſelf, and that it is repreſented by the co-ſine of the angle of inclination of this latter power, they cannot avoid concluding with us, that, ſince the horizontal power has not varied, neither can the magnetic power have varied, unleſs they ſuppoſe that the magnetic power can have varied pre-

cifely

ciſely in oppoſition to, and in the ſame proportion as the coſine of its inclination, which is by no means probable. In ſupport of our concluſion we have, beſides, the experiment on the inclination made at the height of 3863 metres (12651.325 feet), which proves that, at that elevation, the variation did not vary in a ſenſible manner.

Further proofs of the magnetiſm being the ſame as on the earth.

When at the height of 4511 metres, I preſented to a ſmall touched needle, and in the direction of the magnetic power, the lower extremity of a key; the needle was attracted, and afterwards repelled by the other extremity of the key, which I brought down parallel to itſelf. The ſame experiment, repeated at 6107 metres, had the ſame iſſue: another very evident proof of the action of terreſtrial magnetiſm.

Heights at which the air was taken for examination.

At the height of 6561 metres (21487.275 feet), I opened one of my two balloons of glaſs; and at 6636 metres (21733.9 feet), I opened the ſecond: the air entered both with a hiſſing noiſe. Finally, at 3h 11′, the aeroſtat being perfectly full, and having only 15 kilogrammes of ballaſt, I determined to deſcend. The thermometer was then at 9.5° below the temperature of melting ice, and the barometer at 32.88 centimetres; which gives for my greateſt elevation above Paris, 6977.37 metres (22850.887 feet), or 7016 metres (22977.4 feet) above the level of the ſea.

Greateſt elevation.

Effects on the animal economy at this height.

Although well clothed I began to feel the cold, particularly in my hands, which I was obliged to keep expoſed to the air. My reſpiration was ſenſibly obſtructed; but I was far from feeling any inconvenience ſufficient to induce me to deſcend. My pulſe and my reſpiration were greatly accelerated; hence from reſpiring very frequently in a very dry air, I could not be ſurpriſed at my throat being ſo dry, that it was with difficulty I ſwallowed bread. Before ſetting out I had a ſlight head-ach, ariſing from the fatigues of the preceding day and the want of reſt in the night, and I retained it all the day, without perceiving that it was increaſed. Theſe are all the inconveniences I experienced.

Clouds at a very great height during this aſcent.

A phenomenon which ſtruck me at this great height, was the ſeeing clouds above me, and at a diſtance which appeared to me to be very conſiderable. During our firſt aſcent the clouds did not riſe above 1169 metres (3828.475 feet), and above, the ſky was in its greateſt purity. Its colour at the zenith was ſo intenſe, that it might be compared with that of

Their extreme height in the former one.

Pruſſian blue; but in the laſt voyage which I made I ſaw no clouds below me; the ſky was very vaporous, and its colour generally dull. It is perhaps not uſeleſs to obſerve, that on the day of our firſt aſcent the wind was north-north-weſt, and on the laſt, ſouth-eaſt.

Direction of the wind.

Deſcent.

As ſoon as I perceived that I began to deſcend, I thought only of moderating the deſcent of the balloon, and of rendering it extremely ſlow. At 3^h 45′ my anchor touched the ground and held, which gives 34′ for the time of my deſcent. The inhabitants of a ſmall hamlet in the vicinity ſoon ran to the ſpot, and while ſome brought the balloon towards them by drawing the rope of the anchor, others, placed under the boat, waited impatiently till they could reach it with their hands, to catch and depoſit it upon the earth. My deſcent was therefore effected without the ſlighteſt ſhock or the leaſt accident, and I do not believe that a more fortunate one is poſſible. The ſmall hamlet beſide which I deſcended, is called Saint-Gourgon; it is ſituated ſix leagues north-weſt from Rouen.

Analyſis of the air taken at great elevations.

On my arrival at Paris, my firſt care was to analyze the air I had brought. All the experiments were made at the Polytechnic School, under the eyes of MM. Thenard and Greſſet, and I truſted as much to their judgment as to my own. We noticed the diviſions of the eudiometer alternately, and without communication, and it was only when we were perfectly agreed that we wrote them down. The balloon which was filled with air at 6636.5 metres (21735.537 feet), was opened under water, and we all judged that it had at leaſt filled the half of its capacity; which proves that the balloon had retained the vacuum very well, and that no foreign air had entered it. We purpoſed to have weighed the quantity of water entered into the balloon, to compare it with its capacity; but not having the neceſſary inſtruments at hand, and our impatience to know the nature of the air it contained being very great, we did not make that experiment. At firſt we uſed Volta's eudiometer, and we analyzed it comparatively with atmoſpheric air taken in the middle of the court-yard of the Polytechnic School. The following is the comparative analyſis of theſe two airs.

	Analyſis of the atmoſpheric Air.	Meaſures.	*Analyſis of the Air taken at an Elevation of 6636 Metres.*	Meaſures.
Firſt Experiment.	Atm. Air,	3.	Air, - -	3.
	Hyd. Gas,	2.	Hyd. Gas, -	2.
	Reſidue after the combuſtion, -	3.04	Reſidue, -	3.05
2d Experiment.	Atm. Air,	3.	Air, - -	3.
	Hyd. Gas,	2.	Hyd. Gas, -	2.
	Reſidue,	3.05	Reſidue, -	3.04

Atmoſpheric air and air taken at a great elevation, are identically the ſame.

At the ſame time one meaſure of very pure oxigen gas required 2.04 meaſures of hidrogen gas; and this reſult differing only .01 from that obtained in experiments made on a very large ſcale, and with great care, in the compoſition of water, it is evident that great confidence may be placed in our reſults. They prove therefore that atmoſpheric air, and air taken at an elevation of 6636.5 metres, are identically the ſame, and that each of them contains .2149 of oxigen. On analyzing the latter air by hydro-ſulphuret of potaſh, we found in it .2163 of oxigen. I cannot give the reſult of the comparative experiment made on atmoſpheric air, becauſe we were unable to collect it; but the proportion of oxigen which I have indicated, is alſo a little greater than that given by the combuſtion of hidrogen gas, and is comprized within the limits of the variations found in the compoſition of the atmoſphere at the ſurface of the earth, and which do not prevent it from being conſidered as conſtant.

This fact proved by other experiments.

The identity of the analyſes of the two airs made by hidrogen gas, proves directly, that that which I brought did not contain the latter gas; nevertheleſs I alſo ſatisfied myſelf of it, by burning with the two airs only a quantity of hidrogen gas leſs than would have been requiſite to abſorb all the oxigen gas; for I found that the reſidues of the combuſtion of the two airs with the hidrogen gas were exactly the ſame.

Sauſſure

The compoſition of the atmoſphere is every where the ſame.

Sauſſure junior alſo found, by uſing nitrous gas, that air taken in the Col-du-Geant contained, within one hundredth part, as much oxigen as that of the plain; and his father aſcertained the preſence of carbonic acid on the ſummit of Mont-Blanc: Moreover, the experiments of MM. Cavendiſh, Macarty, Berthollet, and Davy, have confirmed the identity of the compoſition of the atmoſphere over all the ſurface of the earth. It may therefore be concluded generally, that the conſtitution of the atmoſphere is the ſame from the ſurface of the earth to the greateſt heights to which we can aſcend.

Recapitulation of the reſults.

Theſe are the two principal reſults which I obtained from my laſt voyage. I have aſcertained the fact which had been obſerved by M. Biot and me, on the ſenſible permanence of the intenſity of the magnetic power at a diſtance from the ſurface of the earth; and beſides, I think I have proved that the proportions of oxigen and azote which conſtitute the atmoſphere, do not vary ſenſibly in the moſt extended limits. Many things ſtill remain to be elucidated in the atmoſphere, and we hope the facts we have already collected will ſo far intereſt the Inſtitute as to induce them to deſire us to continue our experiments.

TABLE OF OBSERVATIONS.

Temperature expreſſed in degrees of the centigrade thermometer.	Mean of the indications of the two hygrometers.	Mean height of the barometer in the atmoſphere brought to that of a barometer with a conſtant level.	Correſponding heights in metres above Paris.	Correſponding heights in Engliſh Feet.	Number of magnetic oſcillations.	Duration of oſcillations expreſſed in ſeconds.	Oſcillations reduced to the common number 10.	Correſponding times.	OBSERVATIONS.
°	°	Cent.				″		″	
27.75	57.5	76.525	0.0	0.0	30	126.5	10	42.16	The firſt line gives the obſervations which were made on the earth before the departure.
12.50	62.0	53.81	3032.01	9929.803	20	83.0	10	41.5	
11.00	50.0	51.48	3412.11	11274.660					
8.50	37.3	49.68	3691.32	12089.073					
10.50	33.0	49.05	3816.79	12499.987	10	42.0	10	42.0	
		45.28	4511.61	14775.522	30	127.5	10	42.5	
12.0	30.9	46.66	4264.65	13966.728	30	125.5	10	41.8	
10.0	29.9	46.26	4327.86	14173.741	20	86.0	10	43.0	
8.25	27.6	44.04	4725.90	15469.322	20	84.5	10	42.2	
6.50	27.5	43.53	4808.74	15746.200	30	128.5	10	42.8	All the heights muſt be augmented by at leaſt 39 metres (20 toiſes), if it is wiſhed to reckon them from the level of the ſea.
8.75	29.4	45.28	4511.61	14775.522	30	127.5	10	42.5	
5.25	30.1	42.49	5001.85	16381.058					
4.25	27.5	41.14	5267.73	17251.815	40	169.0	10	42.2	
2.5	32.7	39.85	5519.16	18069.249					
0.5	30.2	39.01	5674.85	18585.133					
1.0	33.0	41.41	5175.06	13498.321	30	126.5	10	42.1	
− 3.0	32.4	37.17	6040.70	19783.292					
− 1.5	32.1	36.96	6107.19	20001.047	20	84.0	10	42.0	
0.0	35.1	39.18	5631.65	18387.343	30	127.5	10	42.5	
− 3.25	33.9	36.70	6143.31	20119.340	20	82.0	10	41.0	
− 7.0	34.5	33.39	6884.14	22545.558	20	83.5	10	41.7	
− 9.5		32.88	6977.37	22850.887					

XIII.

Experiments and Obſervations on the Motion of the Sap in Trees. In a Letter from THOMAS ANDREW KNIGHT, *Eſq. to the Right Hon.* SIR JOSEPH BANKS, *Bart. K. B. P. R. S. From the Philoſophical Tranſactions for* 1804.

MY DEAR SIR,

Experiments and obſervations on the motion of the ſap in trees.

IN the Obſervations on the Deſcent of the Sap in Trees, which I laſt year took the liberty to requeſt you to lay before the Royal Society, I offered a conjecture, that the veſſels of the bark, which paſs from the leaves to the extremities of the roots, were, in their organization, better calculated to carry the fluids they contain towards the roots than in the oppoſite direction. I had not, however, at that time, any experiment directly to ſupport this ſuppoſition; but I thought the forms generally aſſumed by trees in their growth, evinced the compound and contending actions of gravitation, and of an intrinſic power in the veſſels of the bark, to give motion to the fluid paſſing through them. In the account of the experiments which I have now the honour to addreſs to you, I truſt I ſhall be able to adduce ſome intereſting facts, in ſupport of that inference.

Having ſelected, in the ſpring of 1802, four ſtrong ſhoots of the vine, growing along the horizontal trellis of my vinery, I depreſſed a part of each ſhoot, whilſt it was ſoft and ſucculent, about three inches deep, into the mould of a pot placed beneath it for that purpoſe; but without making any wound, or inciſion, in the young ſhoots thus employed as layers.

In this poſition they remained during the ſucceeding ſummer; and, in the autumn, had nearly filled the pots, which were ten inches in diameter, with their roots. As ſoon as the leaves had fallen, the layers were diſengaged from the parent ſtocks; and about five inches of wood, containing one bud, were left, both at the proper and the inverted end of each layer. Every bud was alſo, by previous management, made to ſtand at an equal diſtance from the mould in the pots, and with an equal elevation, of about thirty-ſix degrees. About one inch of wood was likewiſe left at each end of every layer beyond the buds.

Experiments and obſervations on the motion of the ſap in trees.

In the ſucceeding ſpring, the buds vegetated ſtrongly, both at the proper and at the inverted ends of the layers, as the experiments of Hales and du Hamel had given me reaſon to expect; and, in one inſtance, the bud at the inverted end of the layer grew with greater vigour than that at its proper end: but the growth of theſe buds was not the object which I had in view.

I have already ſtated, that nearly an inch of wood was left at each end of every layer, beyond the bud; and, to this wood, at the inverted ends of the layers, my attention was chiefly directed: for, if the veſſels of the bark poſſeſſed the powers I attributed to them, I concluded that the ſap would be impelled to the inverted ends of the layers, and be there employed in the production of new wood, and roots; and, in this, my expectations were not diſappointed. At the proper end of the layers, the wood immediately beyond the buds became dry and lifeleſs, early in the ſucceeding ſummer; the ſtems alſo, between the buds and the mould in the pots, increaſed in ſize as uſual; and nothing peculiar occurred. But, at the inverted end, appearances were extremely different: new wood here accumulated rapidly beyond the buds, and numerous roots, of conſiderable length, were emitted, whilſt no ſenſible growth took place between the baſe of the young ſhoots and the mould in the pots.

It having been proved by Du Hamel, that inverted parts of trees readily emit roots, I expected to derive further information from cuttings of this kind: I therefore planted, in the autumn of 1802, forty cuttings of the gooſeberry-tree, and an equal number of the common currant-tree; one half of each being inverted. Of the former, not one of the inverted cuttings ſucceeded; whereas few of the latter failed; and in theſe I had an opportunity of obſerving the ſame accumulation of wood above the baſes of the annual ſhoots, and the ſame mode of growth, in every reſpect, as in the inverted vines; except that no roots were emitted at their upper ends. The ſame thing occurred, without any variation, in inverted grafts of the apple-tree.

If it be admitted, according to the theory I have on a former occaſion laid before you, that the ſap deſcends from the leaves through the veſſels of the bark, and that ſuch veſſels are, in their organization, better calculated to carry their contents towards the original roots than in the oppoſite direction, it will be extremely eaſy to explain the cauſe of the accumulation of wood,

Experiments and obſervations on the motion of the ſap in trees.

wood, and the emiſſion of roots, above, inſtead of below, the baſe of the annual ſhoots. The veſſels of the bark (the *vaiſſeaux propres* of Du Hamel) commencing in the leaves, were formerly traced by M. Mariotte, and ſubſequently by myſelf, (being ignorant of his diſcovery,) to the extremities of the roots; and, when a cutting or tree is planted in its natural poſition, the ſap paſſes downwards through theſe, to afford matter for new roots, and to increaſe the bulk of thoſe already formed, having given proper nutriment to the branches and trunk in its deſcent. But, in the inverted cutting, or tree, theſe veſſels become inverted; and, if their organization be ſuch as I have ſuppoſed it, a conſiderable part of that fluid, which naturally deſcends, will be carried upwards, and occaſion the production of new wood, above, inſtead of below, the junction of the annual ſhoot with the older wood, as in the experiments I have deſcribed. The force of gravitation will, however, ſtill be felt; and by its agency, ſufficient matter to form new roots may be conveyed to thoſe parts of the inverted cutting, or tree, which are beneath the ſoil. Beſides, if we ſuppoſe a variation to exiſt in the powers or organization of the veſſels which carry the ſap towards the root, we may alſo attribute, in a great meaſure, to this cauſe, the different forms which different ſpecies or varieties of trees aſſume; for, if the fluid in theſe veſſels be impelled with much force towards the roots, little matter will probably be depoſited in the branches, which, in conſequence, will be ſlender and feeble, as in the vine; and there is not any tree that has been the ſubject of my experiments, in which new wood accumulated ſo rapidly at the upper end of inverted plants. To an exceſs of this power, in the veſſels of the bark, we may alſo aſcribe the peculiar growth of what are called weeping trees; for, by this power, the effects of gravitation will be, in a great degree, ſuſpended; and the pendant branch will continue healthy and vigorous, by retaining its due circulation. The perpendicular branch will, however, ſtill poſſeſs ſome advantages; for in this, gravitation will act on the fluid deſcending from the leaves; and theſe will of courſe abſorb from the atmoſphere with increaſed activity. A greater quantity of matter will therefore enter, within any given portion of time, into veſſels of the ſame capacity; and this increaſed quantity may frequently exceed that which the veſſels of the bark are immediately prepared to carry away. Much new wood will in conſequence be generated,

and

Experiments and obſervations on the motion of the ſap in trees.

and increaſed vigour given; and, the ſame cauſes operating through ſucceſſive ſeaſons, will give the aſcendancy we generally obſerve in the perpendicular branch.

In the preceding experiments, none of the layers, or cuttings, exceeded a few inches in length; and, to the ſummit of theſe the ſap appeared to riſe, through the inverted tubes of the wood, nearly as well as in thoſe which retained their natural poſition. But ſome former experiments had induced me to ſuſpect, that this would not be the caſe in longer cuttings; I therefore planted in the autumn of 1802, twelve cuttings of the ſwallow, *(Salix caprea,)* inverting one half of them. The whole readily emitted roots, and grew with luxuriance; but their modes of growth were extremely different. In the cuttings which ſtood in their natural poſition, vegetation proceeded with moſt vigour at the points moſt elevated; but in the inverted cuttings, it grew more and more languid as it became diſtant from the ground, and nearly ceaſed, towards the concluſion of the ſummer, at the height of four feet. The new wood alſo, which was generated by theſe inverted cuttings, accumulated above the baſes of the annual ſhoots, as in the preceding inſtances.

Theſe facts appear to prove, that the veſſels of plants are not equally well calculated to carry their contents in oppoſite directions; and, I think, afford ſome grounds to ſuſpect that the veſſels of the bark, like thoſe which conſtitute the venous ſyſtem of animals, (to which they are in many reſpects analogous,) may be provided with valves, whoſe extreme minuteneſs has concealed them from obſervation.

The experiments, and ſtill more the plates, of Hales, have induced naturaliſts to draw concluſions in direct oppoſition to the preceding. But the Plates of that great naturaliſt are not always taken correctly from nature; * and Plates, under ſuch circumſtances, however fair and candid the intentions of an author may be, will too often be found ſomewhat better calculated to ſupport his own hypotheſis, than to elucidate the facts he intends to ſtate.

The preceding peculiarities in the growth of inverted cuttings, appear to have eſcaped the obſervation of Du Hamel; and, as very few inſtances of error, or want of accurate obſervation, will ever be found in the works of that excellent naturaliſt, I muſt requeſt permiſſion to ſend you ſome of the ſubjects of my experiments, as vouchers for my own accuracy.

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* The eleventh Plate (Vegetable Staticks) is that to which, in this place, I particularly allude.

Experiments and obſervations on the motion of the ſap in trees.

Of the inverted cuttings employed by Du Hamel, a ſmall portion only appears to have remained above the ground; and under ſuch circumſtances, the different forms of thoſe growing in their natural, or inverted, poſition would be ſcarcely obſervable. It appears alſo, from his experiments, that ſuch inverted cuttings, in ſubſequent years, grow with as much vigour as others that are not inverted; whence we muſt conclude, that the organization of the internal bark becomes again inverted, and adapted to the poſition of the branch. The growth of ſome inverted plants of the gooſeberry-tree, which I obtained, many years ago, from layers, gave me reaſon to draw a different concluſion; for theſe always continued weak and dwarfiſh. I do not, however, entertain the ſlighteſt degree of doubt, but that the aſſertion of Du Hamel is perfectly correct.

I intended to have added ſome obſervations on the reproduction of buds and roots of trees; but theſe would neceſſarily extend the preſent Paper to an immoderate length; I ſhall therefore reſerve them for a future communication, and conclude with an account of an experiment which more properly belongs to the Paper I had the honour to addreſs to you laſt year, but which had not then ſucceeded.

I have ſtated, in that Paper, that the leaf-ſtalk, the fruit-ſtalk, and the tendril, of the vine, had been ſucceſsfully ſubſtituted, in many inſtances, for each other; but that I had failed in my efforts to engraft a bunch of grapes, by approach, on the leaf-ſtalk; owing, I conceived, to the operation having been improperly performed. In thoſe experiments, I cut the leaf-ſtalk into the form of a wedge, and made an inciſion in the fruit-ſtalk, adapted to receive it; but, under ſuch circumſtances, the leaf-ſtalk (as I had proved by many experiments) has no power to generate new matter; and the wounds of the fruit-ſtalk heal ſo ſlowly, that I readily anticipated the ill-ſucceſs of the operation. In the laſt ſpring, I pared off ſimilar portions of the leaf-ſtalk and fruit-ſtalk; and, bringing the wounded parts into contact, I ſecured them cloſely together, by means of a bandage, letting the leaf remain. Under theſe circumſtances, an union took place; and the fruit-ſtalk being then taken off below the point of junction, and the leaf-ſtalk above it, the grapes drew their

Experiments and obſervations on the motion of the ſap in trees.

their whole nutriment through the remaining part of the leaf-ſtalk. They did not, however, acquire their full ſize; and the ſeeds were ſmall, and, I think, incapable of vegetating; but this I attribute to the want of nutriment in quantity rather than in quality; for the union of the veſſels of the leaf-ſtalk with thoſe of the fruit-ſtalk was very imperfect. The grapes, which were the purple Frontigniac, poſſeſſed their muſky flavour, in the ſame degree with others growing on the ſame plant.

There is another experiment in my laſt Paper, which I will alſo notice here, becauſe it appears to lead to ſome important concluſions, and had been tried only in a ſingle inſtance. I have there ſtated, that the ſtem of a young tree became elliptical, by being confined to move only in the ſegment of a large circle. This experiment was ſucceſsfully repeated during the laſt year, on other trees; but I have nothing to add to the deſcription which I have already given.

I am, &c.

T. A. KNIGHT.

XIV.

Extract of a Memoir of Mr. Erman, *entitled, Obſervations and Doubts concerning Atmoſpheric Electricity* *.

MR. ERMAN who is already ſo advantageouſly known to philoſophers, has publiſhed towards the end of laſt year, a memoir which appears to deſerve their attention in an uncommon degree.

Signs of atmoſpheric electricity differ according to the inſtrument.

When he made experiments upon the electricity of the atmoſphere, he obſerved a great difference between the reſult preſented by an electrometer armed with a conductor which was ſuddenly raiſed from the earth, according to the known method, and that of a metallic rod of much greater length which was inſulated and fixed.

Deſcription of the electrometer.

The electrometer he uſed was that diſtinguiſhed in Germany by the name of the electrometer of Weiſs. The length of its leaves of gold is half an inch, and the diameter of the glaſs tube which encloſes them is three quarters of an inch in diameter, the height being an inch and a half. Its cover of ivory does not project above the glaſs, and is perforated in the middle with a hole in which a ſmaller glaſs tube is fixed, and through this laſt tube paſſes the metallic rod that ſerves to ſuſpend the gold leaves, and is upon the whole five feet in length, being compoſed with ſeveral pieces ſcrewed together, in order that they may be more eaſily conveyed from place to place by ſeparating them. A divergence of three lines in theſe leaves anſwers to two lines in the electrometer of Volta; and that degree of electricity which would produce a divergence of two lines and a half in this laſt, cauſes the leaves of Mr. Weiſs to touch the ſides of the glaſs cylinder.

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* Journal de Phyſique, Thermidor 12, or Vol. LIX. 98.

When raiſed ſuddenly it diverges *plus.*

Mr. Erman walked in the fields with his electrometer, having its rod confined to the length of three feet. When he ſuddenly raiſed it from the ground, he obſerved a ſtrong divergence of *plus* electricity. When he lowered it with the ſame ſwiftneſs, he perceived an equal great divergence of *minus.* The point of a fixed metallic rod of much greater length, erected at the ſame ſpot, did not give the leaſt indication of electricity.

If ſlowly raiſed it does not diverge; but if then ſuddenly depreſſed it diverges *minus.*

An electrometer placed on a poſt of three feet in length, produced no divergence; and, when it was afterwards ſlowly raiſed one foot, or a foot and a half, it likewiſe afforded no ſign; but when it was very quickly brought down to the poſt, it exhibited *minus* electricity to ſuch a degree, that the gold leaf touched the ſides of the glaſs. This charge was gradually diſperſed in the air, or by touching the electrometer with the hand*; but by raiſing the inſtrument with equal ſpeed above the poſt, a divergence of *plus* electricity of the ſame ſtrength was afforded which was deſtroyed in the ſame manner.

Conducting power of the air interrupts the reſults.

The more inſulating the air the leſs it is neceſſary to raiſe the electrometer in order to produce this effect; but the longer the point, the more ſtrikingly will the effect be ſhewn, even in damp weather. A circular motion at a conſtant diſtance above the earth does not exhibit any electricity, neither does a progreſſive motion upon level ground afford any change;

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* From this fact it appears, that the electrometer had not any ſlips of metal within the tube like thoſe of Bennet, who firſt uſed gold leaves. N.

but when the ground riſes even inſenſibly, the phenomenon is obſerved, and it is rather extraordinary, that it even affords a means of aſcertaining the level.

Variations of the experiment.

To vary the experiment, the electrometer may be placed upon a low ſupport, and touched with an inſulated ſtick or wire which has been quickly moved towards the ground, and is afterwards raiſed with the ſame quickneſs. The electrometer in the firſt caſe exhibits *plus* electricity, and in the ſecond *minus*.

It is very remarkable, that the *minus* electricity paſſes through Zero before it changes into *plus*, and *vice verſa*, accordingly as the inſtrument is raiſed or lowered even when there is no interval between the two motions; but the divergence will be more viſible, if time be allowed for the electrometer to diſcharge itſelf between the two experiments.

Sauſſure's obſervations.

Sauſſure formerly obſerved the eſſential requiſites in this phenomenon; but his electrometer was not ſo ſenſible, and he explained the phenomenon by ſuppoſing that the electrometer was the moſt inſulated, and the fixed rod not ſufficiently ſo to indicate ſuch ſlight degrees of electricity.

The electrometer is affected not by any electricity in the atmoſphere, but ſimply by the vicinity of the earth which appears to poſſeſs the *minus* ſtate.

Mr. Erman has aſcertained, that the phenomenon muſt be attributed to the manner in which the electricity is diſtributed in bodies. The maſs of the ground exerts its influence very ſenſibly to a certain diſtance, and the electric atmoſpheres act upon each other without any intervention of the air. The inſulated metallic rod which we raiſe from the ground by holding it perpendicularly, undergoes no other change than is owing to its own proper electricity, which is then leſs compenſated by the influence of the ground. Whether the electrometer or the plate of an electrophorus be ſlowly raiſed, the electricity becomes communicated to the ſurrounding air, and is not manifeſted. A ball placed on the point of the rod of the electrometer does not at all change the divergence of the leaves; and even when the conducting rod of the electrometer is encloſed in a tube of glaſs not open at the top, the phenomena are the ſame, and the divergencies of *plus* and *minus* are equally ſeen according to the direction of the motion. And when a ſecond tube is put over the firſt, for the purpoſe of more exactly preventing the contact of the air, the reſults are not at all changed.

The termination of the conductor or even its being encloſed in glaſs, makes no difference.

A fixed rod not affected, becauſe its ſituation with regard to the earth is not changed.

Mr. Erman procured a tube of glaſs 14 feet long, which he covered with varniſh, and through which he introduced a metallic wire which paſſed above the upper part, and had the electrometer adapted to its lower extremity. A ſmall piece of amber being rubbed and applied to the upper end of the wire, produced a divergence in the leaves. The electricity of the air had not, however, any influence on this fixed, and very nicely inſulated point; which ſhews, that the air ſurrounding a point is more adapted to deprive it of electricity than to communicate it. Another electrometer which was raiſed from the earth at the ſame time produced a great divergence.

Two electrometers brought into contact, and then horizontally ſeparated affect each other according to the laws of bodies actually electrified.

Mr. Erman wiſhed to know whether two bodies in the ſame atmoſpheric ſtratum reciprocally modify their electric ſtate, when their diſtance is altered in either direction. Two gold leaf electrometers equally ſenſible, and armed with rods three feet long, one of which was bended, ſo that its point could be placed near that of the other electrometer, on the ſame horizontal line were held at arms length from each other, and touched with the finger to diſcharge, and put them in equilibrium with the ſurrounding air. They were afterwards brought towards each other horizontally, and when they were ſufficiently near to permit their atmoſpheres to act on each other, there was a divergence of *minus* electricity on both ſides, which came to its maximum when the electrometers were in contact. They were again ſeparated, and the divergence diſappeared, both being at the ſtate of Zero. One of the wires of theſe electrometers was put in contact with the earth, when the divergence of the other inſulated electrometer ſuddenly diminiſhed to half its former quantity; a fact that alſo proves that the whole depends upon the electricity inherent in bodies, and not at all upon that of the atmoſphere.

Continuation of the experiment.

When the electrometers were removed from each other, that which was touched with the finger indicated *plus* in a degree equal to the *minus* it had before ſhewn. If the two electrometers be touched with the finger, no further appearance of the *minus* divergence is ſeen in either of them; but if they be then ſeparated both will exhibit *plus*. Can theſe phenomena be attributed to the air? or is it poſſible to avoid admitting in theſe experiments the influence of electric atmoſpheres, and the law of the condenſer?—It is, therefore, well proved, that all bodies, even which are in equilibrium with the earth, have electric atmoſpheres in the open air, from which reſults a modification in their electric ſtate.

The electric influence of a tree was ſtill greater.

Mr. Erman conjectured, that a body of a greater volume might have ſtill more influence. He obſerved a tree which ſtood alone in an open ſpace. He retired to the diſtance of twenty paces from it, and diſcharged the electricity of his electrometer by touching it with the finger. He then approached the tree, keeping his electrometer at the ſame height above the earth. The electrometer diverged negatively, and the divergence continually augmented as he came nearer, ſo that the leaves of gold touched the ſides of the glaſs at the moment when he was under the tree. This negative ſtate laſted as long as he remained there, but when he retired again it gradually diſappeared, ſo as to have become inſenſible when he arrived at the original diſtance of twenty feet.

When the communication between the point of the electrometer and the earth was completed under the tree, the divergence ceaſed; but the electricity was only apparently deſtroyed, and manifeſted itſelf again when he retired from the tree.

The mutual influence of two electrometers is ſcarcely ſeen very near the earth. Remarks on aeroſtatic experiments propoſed.

Two electrometers being brought near each other at the diſtance of half a foot from the earth, did not produce this effect, becauſe the maſs of the earth too greatly influenced that proximity. In order to have very ſimple reſults, it would be requiſite to diſcover ſome means of rapidly elevating an inſulated body to ſome thouſands of feet above the ground; and if this experiment were undertaken in the boat of a balloon, it would probably be ſeen, that the poſitive charge would conſtantly increaſe during the aſcent, and would become negative as it approached the earth: but if means could be had of changing the ſyſtem of the balloon in the atmoſphere, by the intervention of another body, which would be difficult, the contrary effects might be expected. This change of ſyſtem is however ſeen when a cloud deſcends, and diſcharges itſelf with exploſion into the earth. But an electrometer of the uſual conſtruction in the boat of a balloon would indicate nothing, becauſe the upper part always has a charge of electricity of the ſame kind as that of the gold-leaves, and of equal intenſity.

Theſe effects cannot take place in a chamber, &c.

If the influence of the electricity of the ground be ſufficient to prevent the divergence, the ſame effect ought to take place in every cloſe chamber, becauſe a roof or ceiling may be conſidered as a prolongation of the ground. Hence it is that this polarity does not manifeſt itſelf in a chamber; it is alſo erroneous to pretend that the atmoſphere produces electricity; and it is equally erroneous to aſſert, that electric repulſion does not take place in a vacuum; Mr. Erman propoſes to eſtabliſh in an inconteſtible manner in another memoir, that the repulſive force of electricity, as well as of magnetiſm, are alſo manifeſted in a vacuum.

The electric ſigns do not come from the air; for the rod is not alike electrified throughout.

If the electricity of the rod of the electrometer could be aſcribed to that of the atmoſphere, it would follow, that it ſhould be equally electric through its whole length; but if it depend on the earth, the rod will not be equally electrified, and its different parts will be variouſly modified accordingly as they may be more or leſs remote from the earth; and this in fact is the caſe. Two electrometers, each having a pointed rod of three feet, one of which was twice bended in a right angle, ſo as to have an horizontal portion of three or four inches before it proceeded upwards, were held in ſuch a manner, that the point of the ſtraight rod of the one touched the horizontal part or elbow of the other, which was lower. When they were touched, to diſcharge them, no divergence was ſeen, but when they were afterwards ſeparated horizontally, the leaves of the lower electrometer diverged, while thoſe of the upper were not at all moved.

Its ſtate differs as the contact is made in different parts of its length.

It is very remarkable that the nearer the contact is made to the upper point of the compound conductor to take its electricity from it, the greater is the poſitive divergence of the lower electrometer; and if the diſcharge be made very near the upper point, the electrometer will indicate *plus* but very feebly in compariſon with that below.

Electric polarity.

If theſe two electrometers, the conducting parts of which form together a length of ſix feet, be raiſed perpendicularly ſome feet above the earth, the *plus* electricity will be obſerved in both; but if they be ſeparated horizontally, the upper one will retain its divergence, and the lower will augment it ſuddenly, even to double. From this it is evident that the conductor has not the ſame charge of electricity through all its length, and this is preciſely what is called polarity; and as the preſence of the electrometer is not eſſential in this experiment, we may conclude from it that each conductor placed perpendicularly on the earth, has its polarity in the direction in which we have juſt explained it.

One extremity of an electrometer indicates divergence; the other does not.

This ſhows why an electrometer indicates a divergence *plus* when the point is touched with the hand in the open air, and is ſoon withdrawn, while there is no divergence when the lower part of the wire is touched. This phenomenon is ſeen in Weiſs's electrometer, even when the wire is not more than three quarters of a foot long.

Experiments on the electricity of the atmoſphere uncertain.

From all this it will be ſeen how deceitful the experiments on the electricity of the atmoſphere may be; for in the common method, the hand is directed to be moved from the bottom upwards, towards the point of the electrometer, to deprive it of its accidental electricity, and it is preciſely this which communicates it to it.

The intervention of a cloud, rain, &c. reverſes the phenomena;

With reſpect to the exceptions which the facts announced above may be liable to, it muſt be obſerved that when a ſtorm drives a cloud over the zenith of the obſerver, or when rain, hail or ſnow falls at the place of obſervation, the phenomena which occur are totally oppoſite; the electrometer gives *plus* when it is brought towards the earth, and *minus* when it is removed from it, &c. &c. But this anomaly is only a tranſient effect, and the pretended negative ſtate of the atmoſphere does not exiſt, either when the rain is of long duration, or when the ſky is entirely covered with clouds; for in the latter caſe the electrometer is poſitive on being raiſed, as when the ſky is clear. The perturbations ſpoken of are only momentary and change continually. It is evident that theſe changes cannot be attributed to the charge of electricity ſet at liberty in the air: it is more probable that theſe maſſes of clouds modify the electrometer by their atmoſpheres, like the tree in the preceding experiment. The perpendicular conductor may be compared to an iron bar, the polarity of which is reverſed accordingly as the ſame pole of a magnet is preſented to one or other of its extremities; theſe effects of the meteoric maſſes may alſo be imitated by conductors; the ſole motion of the hand above the point of an electrometer is ſufficient to produce the negative ſtate in queſtion, and inſulation renders theſe effects more ſenſible.

but a continued rain or a ſky wholly obſcured by clouds does not.

Theſe changes occaſioned by the atmoſphere of the cloud,

and may be imitated.

(To be concluded in our next.)

SCIENTIFIC NEWS.

Mechaniſm for directing aeroſtatic machines.

PROFESSOR DANZEL of Hamburgh made, in the beginning of laſt year, a ſecond experiment of the mechaniſm he has invented for the direction of aeroſtatic machines. He is ſaid to have obtained a progreſs, in a right line, of twelve feet in a ſecond, which is nearly equal to three leagues in an hour, although he did not bring more than half his means into action. On the ſame day he made an eſſay with another machine for the ſame uſe, the reſult of which was not leſs ſucceſsful. He has ſince publiſhed the principles of his diſcovery in a pamphlet, entitled, "Baſis of the Mechaniſm for the Direction of Aeroſtatic Globes, by Profeſſor Danzel, of the Society of Emulation of Abbeville, at Hamburgh."

Correſpondents elected by the claſs of phyſical and mathematical ſciences of the French national Inſtitute.

The claſs of phyſical and mathematical ſciences of the National Inſtitute of France, in its ſitting of 2d Pluvioſe, in the year XII. elected the following correſponding members. Meſſrs. Leblond, engineer, returned from Cayenne; Bernard, engineer and aſtronomer, at Bagnols; Simons, phyſician at London; Crell, German chemiſt; Thunberg, the ſucceſſor of Linnæus in Sweden, who made the voyage to Japan; Bugge, aſtronomer to the king of Denmark; Goſſe, chemiſt at Geneva; Prouſt, chemiſt at Madrid; Cugnoli, aſtronomer at Modena; Reboul, to whom we are indebted for the meaſures made in the Pyrenees; Mendoza, a Spaniard, ſettled at London, known by his important works on navigation. They were all correſpondents of the Academy of Sciences in 1793.

Prize queſtions

The ſame claſs has prolonged the time for receiving anſwers to the following prize queſtions, until the 1ſt Germinal, in the year XIII. "To determine by experiment the different ſources of carbon in vegetables." And, "To determine by anatomical and chemical obſervations and experiments, what are the phenomena of the torpidity which certain animals, ſuch as marmots, dormice, &c. experience during the winter, with reſpect to the circulation of the blood, reſpiration and irritability; to enquire what are the cauſes of this ſleep, and why it is peculiar to theſe animals."

The value of the prizes is doubled, and conſiſts of two kilogrammes of gold, about 233*l.* ſterling each.

The aſtronomical prize, inſtituted by M. de Lalande, to be given to the perſon, in France or elſewhere, the members of

Prize questions. the Inſtitute alone excepted, who ſhall have made the obſervation moſt intereſting, or publiſhed the memoir moſt uſeful to the progreſs of aſtronomy, was in the public ſitting of the 6th Meſſidor, decreed to M. Joſeph Piazzi, Profeſſor Royal of Aſtronomy and Director of the Obſervatory of Palermo, for the work which he has lately publiſhed under the title: *Præcipuarum ſtellarum inerrantium poſitiones mediæ ineunte ſæculo XIX. ex obſervationibus habitis in ſpecula Panormitana. Panormi*, 1803, one volume in folio.

The ſubject of the mathematical prize propoſed by the claſs, in this ſitting, is the following: To give the theory of the perturbations of the planet Pallas, diſcovered by M. Olbers." The works are to be written in French or in Latin; and are to be received to the 1ſt Germinal in the year XIV. The prize is a gold medal weighing a kilogramme.

The academy of ſciences, literature, and the fine arts of Turin has propoſed the following new prizes.

Claſs of Phyſical and Mathematical Sciences.

Firſt Prize. The electric and galvanic fluids offer ſo many points of analogy, and ſo great a number of different effects, that many philoſophers believe them to be identical, and many others make them two diſtinct fluids:

New experiments are required which ſhall decide, in a definitive manner, on their identity or diverſity.

Second Prize. It will be ſeen in the *Connoiſſance des tems*, for the year XII. page 217, that the refractions which are adopted do not ſhow an agreement in the obſervations of the ſummer and winter ſolſtices of the years 7, 8, and 9, to give the ſame obliquity of the ecliptic, as they ſhould do; and it is clear that a difference, ſuch as is found, of eight ſeconds in the reſult of the calculations, not of one obſervation, or of two, but in the total of ſeveral, made on different days of different years, muſt have ſome cauſe:

A ſatisfactory explanation is demanded.

Claſs of Literature and the Fine Arts.

Queſtion propoſed. *To demonſtrate whether the economical ſcience known by the name of* Statiſtics, *is a new ſcience, and what are the advantages ſtates may derive from it.*

The prize for each queſtion is 600 francs; and memoirs are to be received until the 30th Frimaire of the year XIII. They are to be written in Latin, French, or Italian, and ſent, free of poſtage, to the academy. The prizes will be declared in the laſt public ſitting (Meſſidor) of the ſame year. Prize queſtions.

The ſociety of emulation of Colmar in its ſitting of the 17th Thermidor, propoſed the following ſubject for a prize of 300 francs, to be decreed in the public ſitting of Meſſidor, in the year XIII.

"1. What are the readieſt and leaſt expenſive means of converting into animal oil, ſuch remains of animals as have been hitherto loſt to the arts and to conſumption?

"2. To what arts, and to what kinds of fabrication can this product be applied; and what preparations ſhould this ſubſtance undergo before it is brought into commerce?

"3. Finally, in an extenſive undertaking, the object of which would be to convert the remains of animals into animal oil, what precautions would be neceſſary in the adminiſtrative police, to prevent the public health from being injured?"

The memoirs are to be written in French, German, or Latin, and ſent before the 13th Meſſidor, free of poſtage, to the Prefect of the Department, or to the Secretary of the Society.

Extract of a Letter from Mr. J. DALTON, *Lecturer at the Royal Inſtitution. Dated, Mancheſter, March* 12, 1805.

A VERY remarkable and ſingular appearance of the *aurora borealis* was obſerved at Mancheſter from 10 to 11 in the evening of Saturday the 23rd of February. The ſky was almoſt completely obſcured with dark clouds, eſpecially to the ſouth, with ſome rain; about 60° above the horizon, on the ſouth meridian, there was a ſpace in which the cloud was leſs denſe, where a very ſtriking vacillating flame arreſted the attention of moſt people who happened to be out. It ſometimes ſhone with ſuch vividneſs as to exhibit a ſtream of light right down to the horizon through the thickeſt part of the cloud; and at other times, the whole ſouthern region was illuminated as with a flaſh of lightning. The light ſeemed ſcarcely to reach the zenith, and at breaks in the clouds to the north, no light appeared. Should this phenomenon have been obſerved in the ſouth of England or in France, a compariſon of the obſervations may lead us to form ſome idea of the height of this and other appearances of this meteor, which occur much more rarely of late years than formerly. Remarkable aurora borealis.

To

THE ingenious inſtrument of I. S. F. *for exploſions has not appeared to the Editor to be wanted. Chemical lecturers in this metropolis uſe a ſmall particle of phoſphorus, of the ſize of a pin's head, and ſprinkle upon it a pinch of the oxymuriate of potaſh, in a Wedgewood's mortar. This ſmall quantity, being ſtruck by a ſudden rub of the peſtle, gives a loud exploſion with the* viſible *flame, but the burning matter is too little to endanger the ſpectators.*

The excellent perſpective inſtrument deſcribed and drawn by the Rev. WM. GREGOR, *has no doubt the priority in actual date to that of* R. L. EDGWORTH, *Eſq. deſcribed in our firſt Vol. page* 231. *But he will ſee, by turning to that article, that the inſtruments are too nearly alike to admit of a ſecond publication.*

I am much obliged to Mr. CUMBERLAND *for his favour of the* 17*th Feb. The uſeful objects mentioned in his poſtſcript appear to me to be ſuch as would be very acceptable to the public. I am afraid that the circumſtance of the invention of Mr.* J. *having been publiſhed before would make it leſs proper to be reprinted in a Journal which is honoured with ſo much original matter, that preſſes for inſertion. But I ſhall be happy to give it a more deliberate conſideration if his friend will favour me with the loan of the plates, and ſuch additional obſervations or notes as he may think fit to make.*

The caſe of deafneſs, communicated by Mr. HILL *of Wells, Norfolk, in which ſo much benefit was derived from electricity, is very intereſting. Some medical caſes have appeared in the early numbers of the firſt ſeries of this Journal; and others preſented themſelves for my deciſion as Editor with regard to their value and importance. But in the attempt to perform this part of my duty, I found difficulties ſo great, and a ſubject ſo extenſive, that it appeared proper to confine the papers, to be admitted, to ſuch as are directly and immediately connected with chemical and mechanical ſcience. I ſhall be happy to convey his letter to a reſpectable Medical Journal, if he ſhould think fit.*

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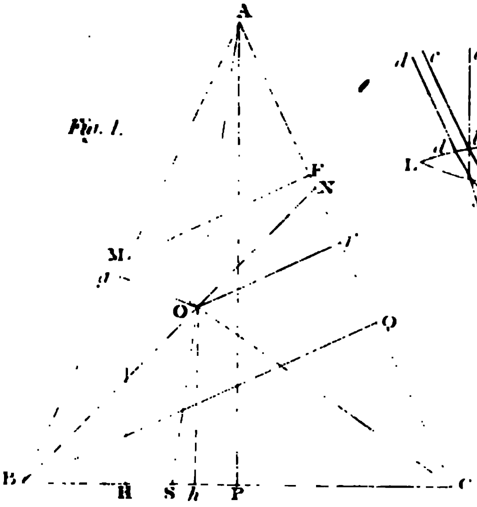
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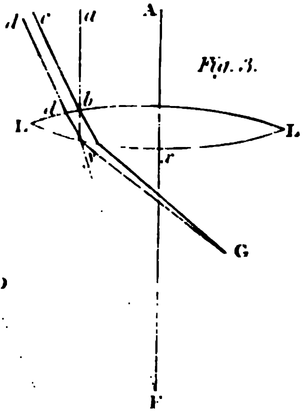
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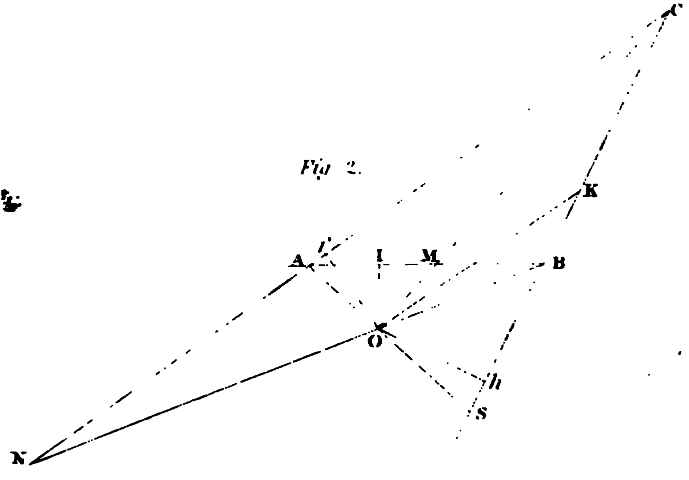
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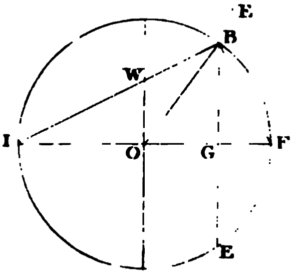
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Philos. Journal. Vol. X. Pl. XI. p. 304.

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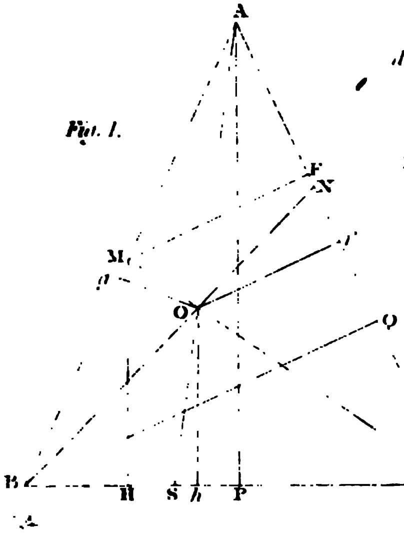

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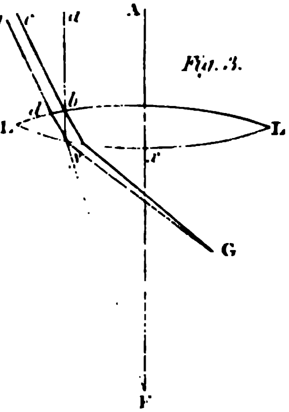

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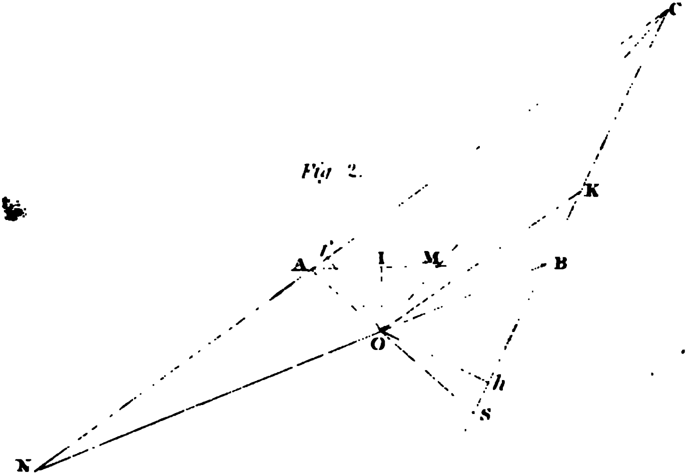

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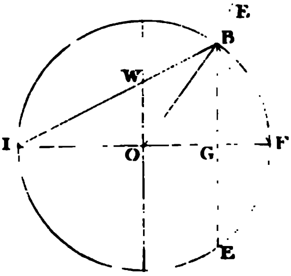

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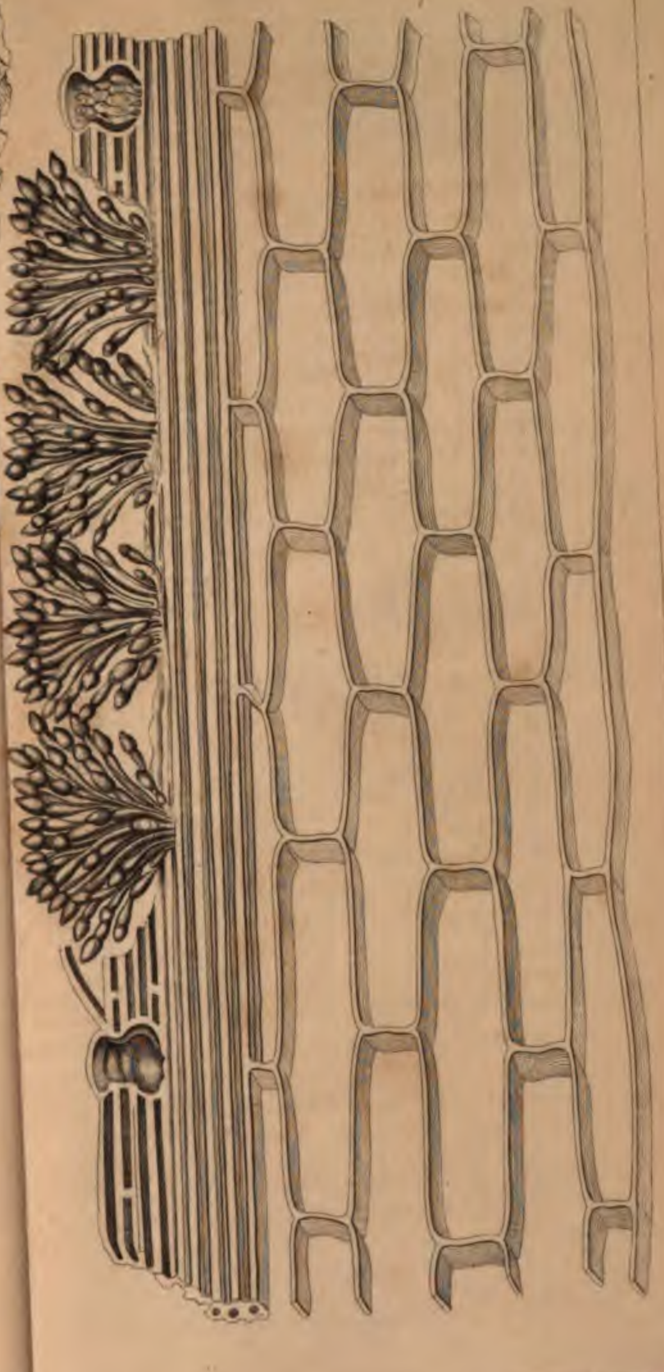

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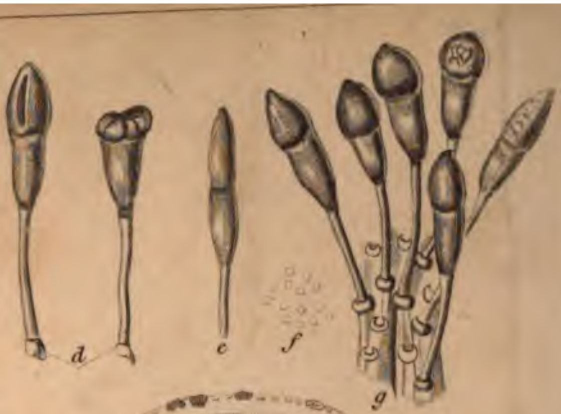

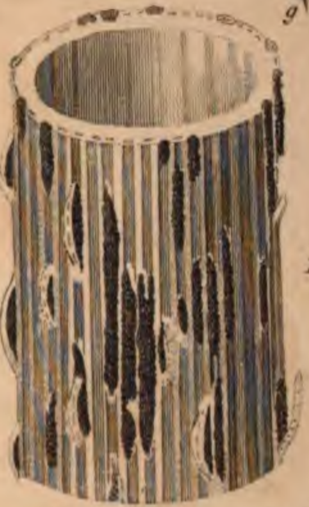

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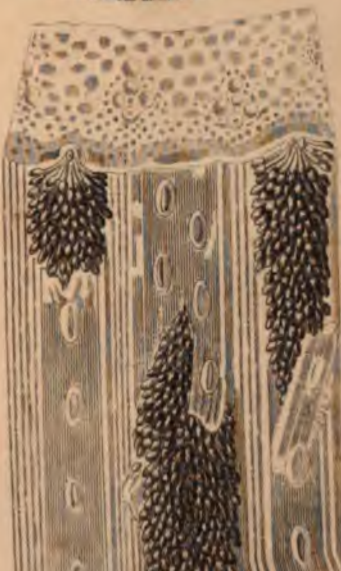

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W. Stratford, Printer, Crown Court, Temple Bar.